The British Journal for the
Philosophy of Science

VOLUME II AUGUST, 1951 No. 6

THE SCIENTIFIC OUTLOOK IN 1851 AND IN 1951 *

' A THOUSAND years in thy sight are but as yesterday when it is past.' On this scale, which recognises the proportions no more than the unit measures of astronomical time-keeping, we may place the year 1851 at 11 o'clock yesterday morning and the present moment shortly after dawn today. The day so near its culmination when the Great Exhibition displayed and concealed its glories woke to life in the seventeenth century, with the heliacal rising of Galileo, shortly to be followed by the sun of Newton. The day that succeeded it opened in cloud, through which rays from the sun of Einstein are now beginning to light our way. But we do not get a new sun with each new day. The old one returns,

> repairs his drooping head,
> And tricks his beams, and with new spangled Ore,
> Flames in the forehead of the morning sky.

The sun of Einstein is the sun of Newton, and we, awakened and refreshed, seeing the shadows shortening instead of lengthening, nevertheless walk by the same light as did those who began the scientific pilgrimage yesterday morning.

That, in substance, is what I want to say. And now, since the Astronomer Royal holds high office in this Institution, I must return from eternity to time and fit it into one of his mean hours.

What I am concerned with this evening is not the actual state of scientific knowledge today and in 1851, or the actual or possible

---

* A Discourse given at the Royal Institution, London, on Friday, 2nd March 1951.

applications of it to practical affairs, but the scientific outlook, the ideas of what they were and are doing held by those responsible for the creation of that knowledge as well as by the general public. All, therefore, that I need say about the visible content of science in 1851 is that it was firmly grounded on the mechanical ideas of Newton, which seemed to be justified and exemplified by every new advance, and that it accorded perfectly, and apparently inevitably, with the general scheme of things that formed the scientific philosophy of the time. That philosophy is our main consideration, and it would be very satisfactory if I could bring before you some firsthand statements of it by those whose thoughts and actions it directed. That, however, is just what I cannot do. It was fundamental and it was universal, but it was so deep-seated that only on the rarest occasions did it appear in explicit statements. *The Proceedings of the Royal Institution* first appeared in 1851. One can read the early volumes and find excellent accounts of the progress of science in various fields, but no account at all of what science was conceived to be or what its discoveries signified. I must therefore rely for the most part on what seems to me to be implied rather than stated by the scientific literature of the mid-nineteenth century, restricting my references largely to the outstanding, if not the only, work that made the philosophy of science its theme—Whewell's *Philosophy of the Inductive Sciences.* This work was first published in 1840. A second edition appeared in 1848, and there were further editions after the Great Exhibition. Whewell, of whom it was said that science was his forte and omniscience his foible, was not a typical scientist, but he cast his net wide. Supremely confident in his own judgment, he was nevertheless preserved from the more serious consequences that befall such philosophers by the fact that he had previously written a *History of the Inductive Sciences.* There is no easier way to construct a completely self-satisfying philosophy of science than to be ignorant of history, as our own time amply demonstrates. Whewell at any rate wrote with knowledge, and he did succeed in making articulate the instinctively-held convictions of the time.

The basic assumption of the scientists of 1851 was that there was lying before them a world of material objects, moving about in space and time. Their task as scientists was to study this world by direct observation, by the use of instruments, and by experimental arrangement of conditions so as to facilitate observation and measurement. None of these processes was conceived to change the world in any way.

Whether it was observed or neglected, known or unknown, understood or not understood or misunderstood, it remained the same independent world that had existed before there were minds to apprehend it, and would go on existing and operating in exactly the same way if all life ceased to be. There was, in fact, a fundamental dichotomy between the material world on one hand, and, on the other, human minds with their hopes and fears, despairs and aspirations, exultations and agonies, disappointments, convictions, illusions and everything that makes intelligent life what it is. None of these things was any concern of science. They did not exist in the sense in which the material world existed. They were ephemeral, incalculable and, from the scientific point of view, negligible.

It is important to realise the strength and the essentially metaphysical nature of this belief in the existence or reality or substantiality, or whatever you care to call it, of matter, because in these days of return to the strictly empirical basis of science, those more at home with the modern outlook (fortunately for my purpose now, they are not numerous) might not realise exactly what it meant. It implied that any particular piece of matter—say a stone—though it was made evident to us by its qualities—its hardness, colour and so on—was nevertheless not compounded of those qualities but was some underlying entity of which they were merely accidents. This essential stone indubitably *existed*, and although, if pressed, the scientist might have found it difficult to say exactly *what* existed, or what *existence* meant, or why he was so sure that the stone existed, in fact he rarely if ever was pressed, and so he had no reason to call his assumption into question. Whewell expresses the matter very clearly :

> The Idea of Substance . . . is involved in all our views of external objects. We unavoidably assume that the qualities and properties which we observe are properties of *things* ;—that the adjective implies a substantive ;—that there is, besides the external characters of things, something *of which* they are the characters. An apple which is red, and round, and hard, is not merely redness, and roundness, and hardness; these circumstances may all alter while the apple remains the same apple. Behind the appearances which we see, we conceive something of which we think; or to use the metaphor which obtained currency among the ancient philosophers, the attributes and qualities which we observe are supported by and inherent in something : and this something is hence called a substratum or *substance*, that which stands beneath the apparent qualities and supports them.

Digitized by the Internet Archive
in 2023

https://archive.org/details/british-journal-for-the-philosophy-of-science_1951-08_2_6

The scientific quest, then, may be summed up as the search for the universal, inviolable, causal laws that governed the course of events in the real external world. The general character of the laws was known ; they were descriptions of the operation of forces—or, in general, agencies—on a world of inert ponderable matter. The matter could be observed ; the agencies had to be inferred from the observations. A great part of the work had been done by Newton. His successors had added to his laws of mechanics laws of heat, of light, of sound, of magnetism and electricity, and all these had either been reduced, or were believed to be reducible, to the fundamental mechanical laws. The completion of the process was in sight, and the next generation or so would see the whole round world in every way bound by causal chains about the feet of Newton. It was the eleventh hour of scientific progress, and it brought with it appropriate emotions. ‘ We range with Science,’ wrote Tennyson, ‘ glorying in the time.’

Contemplating this situation in the perspective afforded by the passage of a hundred years, we are perhaps most struck by the fact that those who occupied it thought that it was the culmination of the scientific revolution inaugurated by Galileo and established by Newton. Actually it was nothing of the kind. The essence of that revolution was the substitution of the examination of experience for speculation about the external world. In the eighteenth and nineteenth centuries scientists went on examining experience, but imagined that they were still learning about an external world. But in fact such a world had nothing at all to do with scientific practice ; it was an idea unconsciously carried over in thought from the previous centuries. When Galileo rolled spheres down an inclined plane and derived the law of descent, the law said nothing at all about the spheres. It gave a relation between the reading of a measuring rod and that of a water clock ; no reference to a sphere entered into it. The size, colour, temperature, and every other quality of the sphere were quite irrelevant ; even its weight—especially its weight, for that was the point at which the new procedure broke most obviously with the old. It had previously been held that heavy bodies fell faster than light ones ; Galileo showed that weight did not enter into the matter. And Galileo’s independence of the substantiality of the external world was equalled by Newton’s independence of causal laws. ‘ To derive two or three general Principles of Motion from Phænomena,’ he wrote, ‘ and afterwards to tell us how the Properties and Actions of all corporeal Things follow from those manifest Principles, would be a very great step in Philosophy, though the Causes of those Principles were not yet discover'd.' His own great achievement, in fact, was precisely of that kind.

To find the origin of the nineteenth-century philosophy of science we must go back beyond Galileo, and we shall then find it wherever we look, for it dates from the beginning of man's realisation that experience is not essentially chaotic. To take merely one example of each of its two main elements, we find Avicenna, early in the eleventh century, criticising the alchemists on the ground that although they might change all the *qualities* of lead into those of gold, the *essential nature* of the substance still remained that of lead. Avicenna's ' essential nature ' was Whewell's ' substance.' ' Cause ' again is analysed minutely by Aristotle, and the existence of a First Cause deduced. All this, which the seventeenth-century scientists exchanged for the study of experience, the Victorians enshrined in the heart of their thinking, and while making unprecedented strides in the correlation of experience by following the new scientific procedure, they automatically translated their achievement back into the terms of a barren philosophy and praised Newton for what he had disclaimed.

The result of all this was that they were completely deceived about the extent and nature of their achievements. They thought they were approaching the end of their task of discovering the causal laws that governed the world of material substance : actually they were at little more than the beginning of their task of understanding the world of experience. Thinking they knew almost everything, they in fact knew next to nothing. Take the experience of an ordinary day, and consider how much of it they had brought within the scientific scheme. In the first place, about a third of it never entered their heads as either deserving or admitting of understanding. Though dreams had been known to change the course of the world's history, and were indubitably experiences, they were not parts of the material world, and so were denied ' reality.' The enlightened scientists of that time did not know why they preferred eggs to bacon for breakfast ; why an intelligent man like Mr Smith could put that Conservative bill in his window when the Liberals were so obviously the right Party ; why it should be raining again when the weather seemed so settled yesterday ; what nice young Mr Brown could possibly see in that silly Miss Green ; why they should be going to catch a cold when they had carefully avoided all draughts ; why this was such a bad year for apples ; why, in fact, anything at all that happened in the ordinary course of events should happen as it did. Their knowledge was almost wholly confined to what could be observed in circumstances so extraordinary that they never existed at all in the natural course of things. They needed carefully constructed instruments, a laboratory strictly guarded from outside interference, and workers trained by long and specialised practice before anything could be observed that revealed the supposedly universal and inescapable causal laws. Everything that made ordinary familiar occurrences differ from laboratory events was a disturbing influence, to be cleared out of the way before one could observe the handiwork of the Great First Cause. The reason why the Victorians thought they had solved most of their problems was that they had succeeded in persuading themselves that the difficult ones were unimportant. When you have banished everything from the universe except the contents of a scientific laboratory, it is easy to range with Science, glorying in the time ; there's nothing to stop you.

But fortunately the scientists, though they bowed the knee to Baal, went on worshipping Jehovah in their hearts. Their practice was that of Galileo and Newton, though their philosophy was that of the ancients, and sooner or later an incongruity was bound to show itself. It appeared first in the theory of heat. Soon after the time of which we have been speaking, the real, substantial, material object had been recognised as an aggregate of minute identical molecules moving about and constantly colliding with one another. The molecules were not, in fact, observed ; like gravitational force, they were invented, in true scientific fashion, as a medium for expressing what was observed. But the nineteenth-century scientists could not think in terms of experience. Their molecules were unimaginable without the hall-mark of substantiality, so they immediately became bits of matter and were automatically subjected to causal mechanical laws. What those laws were, however, was mysterious. If the molecules obeyed the ordinary mechanical laws of motion it was difficult to account for the variety in the specific heats of bodies. If they did not obey those laws, how could one discover what laws they did obey ? The scientists, at least temporarily baffled by this problem, turned their attention to what could be discovered with the knowledge they did possess, and soon realised that this was quite a lot. They could account for the gas laws, the phenomena of diffusion and transpiration and viscosity ; they could predict surprising things like the independence of the thermal conductivity of a gas on its density, and achieve a number of other results of the greatest interest, and all without knowing in the least how a single one of their molecules behaved or what principles governed its movements. The molecules could, within very wide limits, do what they pleased ; the measuring instruments would give the same readings in any case. It was as though men were learning all about the redness and roundness and hardness of the apple without knowing anything at all about its substantiality or the eternal, immutable laws by which the Great Creator and Designer had left his impress on the universe.

There should, of course, have been protests—reactions of the strongest indignation at this prostitution of the capability and godlike Reason that had been given to men for higher ends. But there were not. So complete was the break between what the scientists were doing and what they thought they were doing that, on the contrary, everybody was delighted. Men's unconscious scientific instincts were satisfied by the progressive correlation of experience, and the demands of their conscious philosophy were met by the confident but quite unfounded hope that somehow all this would lead in time to an understanding of the real, eternal world of molecular movements. It would be an understatement to call the molecules a Mrs Harris : they were the holy St Harrises, worshipped with greater and greater adoration the more they hid themselves from sight. ‘ Though in the course of ages,' wrote Maxwell in 1873, ‘ catastrophes have occurred and may yet occur in the heavens, though ancient systems may be dissolved and new systems evolved out of their ruins, the molecules out of which these systems are built—the foundation stones of the material universe—remain unbroken and unworn.' But if anyone had ventured to say, ‘ Show me the foundation stones, that I may tread on them,' what he would have been shown would have been a demonstration that no matter how you treated a given quantity of gas, a certain function of the readings of thermometers, pressure gauges and metre rods, when applied to it in certain ways, remained constant. *That* was what remained unbroken and unworn. The real, eternal, substantial molecules were figments of the imagination. Such as they were, they were broken with ghastly effect at Hiroshima, but the relation between the measurements was not altered.

This divergence between scientific achievement and the estimation of scientific achievement has grown continuously from that time onwards, until now it is well nigh complete. The more we have learnt about the relations between our experiences, our observations, the less we have been able to say about the supposedly real entities whose actions we have been said to be observing. The simple molecules of Maxwell have become the electrons, protons, neutrons, positrons, π-mesons, μ-mesons, and goodness knows how many more such mysteries, of present-day nomenclature. Maxwell could at least say that his molecules were hard, elastic, isotropic, and a few other things ; all we can say about our foundation stones was summed up in a phrase by Eddington ; ' Something unknown is doing we don't know what.' In another connection, Artemus Ward is reputed to have said : ' The researches of many eminent scientific men have thrown so much darkness upon the subject, that if they continue their researches we shall soon know nothing.' In the matter of the substance of the real, material universe, this ultimate accomplishment has now been literally realised : we do, in fact, know precisely nothing at all.

This path to the revelation of the Victorian misconceptions, as I said, began to diverge from the highway of thought not long after the Great Exhibition of 1851, but it is only in our own time that it has led us to a point from which return to the substantial material world is clearly seen to be impossible. Progress has been gradual, the ultimate particles, while losing more and more of their accessibility, still retaining their hold on our formal belief until almost the present day. But while the process was going on, another quite independent line of research shattered the whole fabric of material substantiality at a single blow. Einstein's special theory of relativity first came before the scientific world in 1905. It showed, first, that there was no sense in which we could say, as an independent fact of nature, that a body had any particular state of motion. We could regard it as stationary or as moving in any direction with any speed ; the choice was entirely ours. Secondly, the theory showed that all the quantities with which we were concerned in physics—the masses, weights, volumes, shapes, temperatures, colours, and so on, of bodies—were dependent on the motions of the bodies. It followed inevitably that all these things also were at our choice ; there was not one single characteristic of one single constituent of the substantial material world that was not subject to our caprice to evaluate it as we would. Everyone has now for many years accepted the theory, but, so tenacious are we of our prejudices, that there are few who realise how completely incompatible it is with any form of materialism. But let us take a single example, and confine ourselves to the so-called fundamental property of matter, its weight—or, since what we measure with a balance and call weight is really mass, to the one property of mass. Imagine two bodies which, when they are lying beside one another, relatively at rest, are exactly alike in every respect—say, two identical copper spheres. When weighed on the same balance in the same laboratory, they are found to have the same mass. Now let one of them, without being otherwise altered in any way, be set in motion with respect to the other. Will their masses still be the same ? Everyone who accepts the principle of relativity will agree that they will not. Which will have the larger mass ? Again, all relativists will say : 'That which is moving.' But which one is that ? Again our relativists will reply : 'Which you choose.' If, then, I choose to say that A is moving and B at rest, than A is the more massive. If, without doing anything at all to the bodies, I then change my mind and say that B is moving and A at rest, then B immediately becomes the more massive. It is thus entirely a matter of your or my caprice which of the two bodies contains the more of the eternal, independent, substantial reality of the universe, and you can make one choice and I another with perfect propriety, while a third person can choose to have the masses equal if he will.

This argument is extremely simple and quite inescapable, and, like the culmination of the study of fundamental particles, it completely destroys the doctrine that what science is examining is an independent world of matter. We still use the language appropriate to such a notion, but it has become metaphorical, and the real subject of our studies and our discoveries is simply our experience. Looking back, we can see clearly enough that it has always been so. The quantities occurring in our equations are the numbers we observe on the scales of our instruments when we perform certain operations with them. It is a gratuitous addition to suppose that they are properties of some metaphysical stuff that we invent to adapt our thinking to the habits of childhood. Up to recently the addition has been more helpful than harmful ; now it has become more harmful than helpful. It is like the carrot held in front of the donkey. So far it has served its purpose. It has made him run heroically and achieve what would have been impossible without its allurements. But now he is getting hungry, and it seems curiously ineffectual in meeting the new need. He is beginning to realise that the road he had been following is not, after all, that which leads to assimilation of the carrot but has been determined by quite other agencies and is directed towards quite another goal. The sooner he learns to focus his eyes on a point beyond the carrot, the sooner will his desires find satisfaction.

To those who succeed in taking the new view, the spectacle of the scientific adventure becomes transformed. We are not, as the nineteenth century thought, near the end of a description of a material world. We are little past the beginning of an understanding of the unity in diversity of our experience, and see an unlimited vista of fresh relations waiting to be understood. The change is fundamental and inescapable, but prejudice dies hard, and it is far from being generally realised. The language of the old material philosophy survived Newton and it is surviving Einstein, though it is now strained almost to breaking point. The philosophy of science is not a popular subject, and for the most part scientists, knowing that they are acting rightly and producing results, give little thought to the meaning of their practice. They are content to let their findings seem nonsensical. Some of them even deprecate such thought, as a distraction from the important business of getting something done. They are willing to see truth for ever on the scaffold, wrong for ever on the throne, so long as the scaffold goes on swaying the future. This attitude is understandable, and if anyone holds such feelings strongly he is undoubtedly right to avoid the philosophy of science ; he has little faculty for it, and his contribution could be spared. But unfortunately the prosecution of science is not the whole of our activity in this vale of tears, and if we wish to see life steadily and see it whole we must take account of fields of action in which a true understanding of what science is doing is of the utmost importance. We may ignore such fields, but they will not ignore us, and unless our ideas are clarified concerning the nature and possible scope of science we are likely not only to find the science of the future becoming more and more unintelligible, but also to wake up too late to the fact that there will be no more science for us to try to understand.

One of the most prominent elements of present-day thought is the so-called philosophy of dialectical materialism. It appears to have originated in a combination of certain economic doctrines, deduced from information available at the middle of the nineteenth century, with the fashionable philosophy of the time—that of Hegel. But the idealistic flavour of Hegel's ideas was little to the taste of the early dialectical materialists, and they substituted for it the current scientific philosophy of a fundamentally real material world which I have been describing. The result was a system in which the basic substance, so to speak, was nineteenth-century matter, and the dynamic was the Hegelian dialectic which played in the world of social and economic affairs a role roughly corresponding to that of Newtonian forces in the world of mechanics. It was an ingenious system, and it assumed its full share, if not more, of the mid-nineteenth-century confidence in the indubitable rightness of its fundamental principles. All that future history could do was to demonstrate their working ; it was powerless to alter them or set them aside.

Anyone who has studied nineteenth-century science can see how conformable this was to the existing scientific philosophy, and anyone who studies twentieth-century science can see how uncomformable it is to ours. Yet the curious fact is that a number of present-day scientists—some of them even physicists of high eminence—embrace the doctrines of dialectical materialism with an enthusiasm suggestive more of the loyalty of a football partisan than of rational conviction. They claim also to find support for it on scientific grounds, and indeed go so far as to say that it is itself a scientific philosophy—the only legitimate one—though of course without the liability to modification that scientific theories are usually held to enjoy. It is appropriate therefore to ask those who advance this view how they reconcile their doctrines with the facts I have been describing—the gradual recession of the substantial material world into complete inaccessibility, and the dependence of the supposed measures of its properties on our arbitrary choice ; for they are, of course, aware of these facts and might be expected to have something to say about them. That, however, appears not to be the case : if one is foolish enough to ask such questions he is left to answer them for himself. As an earnest seeker after truth I have looked in the writings of our scientific dialectical materialists for enlightenment in this perplexing situation, but I have found none at all. When the discussion takes such a turn that the subject of relativity comes into the reader's mind, he can almost see the shocked face of the author start up from the page and hear him say with righteous Victorian horror, 'O no, we never mention her!' And, sure enough, one is soon drawn back to the internal struggle that goes on in a piece of ice when the temperature rises—a subject that is treated in a manner which the most profound student of the caloric theory could hardly improve upon.

What is the meaning of this strange silence ? The question would be of merely academic interest were it not that the false idea that dialectical materialism is a tenable philosophy is a most serious menace not only to the future of civic freedom but also to the future exercise of scientific thought and practice. With the former danger I am not here concerned, but with the latter I am, and I therefore appeal to those scientific men who hold that dialectical materialism has any living importance for us today, to take these specific facts that I have been describing and show us how they can be reconciled with it. Until they do so it is impossible for anyone acquainted with the actual situation to see this strange pseudo-philosophy as anything but a preoccupied and bewildered ghost, wandering aimlessly among the antimacassars and aspidistras and panting for the heavy lavender-laden air in which alone it can breathe freely. Ghosts are interesting things and worthy of serious study ; it is when they are thought to have the properties of living beings that they become terrible.

Of the many aspects of the change from the nineteenth to the twentieth century outlook that would well repay consideration, I can mention only one or two. In general terms we may say that the Victorians looked on the progress of science as a process of accumulation. Knowledge once acquired remained for all time ; fresh knowledge was added to it, but without changing it in any way.

> 'Let Knowledge grow from more to more.'
>
> 'Slowly the Bible of the race is writ,
> And not on paper leaves or leaves of stone ;
> Each age, each kindred, adds a verse to it.'

The process would go on until God had made the pile complete, and it was then near enough to completion to show the essential character of the structure of the universe ; further additions would improve the symmetry without altering the general plan. Our view today is very different. Certainly experience grows from more to more, and must do so as long as time endures and records are preserved ; but the picture of the whole which we form in our attempt to express its interrelations undergoes unceasing transformations—it is a kaleidoscope rather than a fixed mounting structure. Progress in scientific conceptions is like a view gradually emerging out of a drifting mist on which a changing light is playing. We observe what happens when α-particles are projected towards a gas, and our picture of the atom suddenly changes from the likeness of a billiard ball to that of a solar system. We add to our knowledge of the actual solar system the fact that light is deflected by the Sun, and immediately the lines of gravitational force that previously held its members together disappear, and each planet is seen to travel freely along its own groove. We can no longer say, The world is like this, or The world is like that. We can only say, Our experience up to the present is best represented by a world of this character ; I do not know what model will best represent the world of tomorrow, but I do know that it will coordinate a greater range of experience than that of today.

There is another aspect of the new outlook that is of the greatest importance. When we take experience itself as the subject-matter of science, we can no longer maintain the fundamental distinction between sensations and other experiences that seemed so obvious in the nineteenth century. Sensations were then naturally marked out as valid clues to reality because they were revealed by the action of the real external world on our sense organs. Macbeth's dilemma about the reality of his dagger, which he could see but could not feel, was no dilemma to the nineteenth-century playgoer. If the dagger had been real he would have felt it ; he didn't feel it ; therefore it was not real and therefore it could be ignored. It didn't occur to the scientists of that time that, ' real ' or not, Macbeth certainly had the experience of seeing it, and that experience called for scientific study just as much as one accompanied by the corresponding experience of touch.

But the instinct that led the physicist to act scientifically in spite of his unscientific thoughts was no less active in the physiologist and the psychologist. The illusions and hallucinations and dreams that were dismissed with such contempt from the world of reality were nevertheless studied and became the source of far-reaching generalisations of which today everyone realises the importance, whether or not in their present form they are likely to survive further study. The word ' delusion' to a modern psychologist carries the same measure of significance as the word ' reality ' to a spectator of the Great Exhibition. And it is unquestionable that such experiences are significant. Hitler's delusions have caused immeasurable suffering and given a new direction to the course of events all over the world—quite a respectable performance for things that didn't exist. Moreover, in the realm of mental phenomena relations are found which would have been thought fantastic in the nineteenth century. Mr Smith's neighbours, and Mr Smith himself, would have been astonished to learn that his predilection for the Conservative Party had less to do with its political programme than with the fact that a hated schoolmaster had once spoken approvingly of Mr Gladstone ; and Mr Brown

so bitterly argued have been settled. It is rather because those concerned to uphold the validity of religion have realised that the matter has not the importance for them that their grandfathers thought it had. They have seen that it is possible to concede the geologist's time-scale and the gradual evolution of species without in the least impairing the foundations of their religious beliefs, and accordingly they have now no incentive to interfere with the scientist's correlation of his observations of fossils. From the modern point of view, from which experience appears as the starting-point, the initial datum, of our considerations, and not as a derivative of an imagined real material world, the experiences that have given rise to the physical and biological sciences are seen to be different from, but, for scientific purposes, coequal with, those that have given rise to the faiths for which men have died. The correlation of the former, though by far the more advanced, has not yet come within sight of conceptions sufficiently generalised to be capable of embracing the latter.

But again in this field we observe the phenomenon we have noticed before, that the formal philosophy of our men of science lags behind the necessary implications of their practice, and we still have the same old ghosts of Victorian misconceptions haunting the structure of modern scientific philosophy. In a recent popular series of broadcasts, for example, the speaker, in summing up his conclusions on the nature of the physical universe, remarked that ' it seems to me that religion is but a blind attempt to find an escape from the truly dreadful situation in which we find ourselves.' What the ' dreadful situation ' was he did not explain, nor did his tone suggest that he was in fact overcome with dread. Indeed, the conformity of his language to his ideas contrasted somewhat unfavourably with that of the ancient Hebrew writers whom at the time he was patronising : one did not get the same impression of absolute conviction born of living experience. But the chief point of interest is the utter lack of awareness of what scientific progress has been revealing during the last hundred years. To hear religion described as a blind attempt to escape from the conclusions of astronomical research is to hear a blind man, rising from the study of acoustics, pronounce his opinion that optics is an attempt to escape from the truly dreadful jazz band across the way. The cases are precisely parallel. Such nonsense is intelligible only on the assumption that the speaker has not yet realised that science begins with experience, but still presupposes the old substantial material world that reveals itself only to the animal senses.

It should be possible, when these fantasies have been outgrown, for science and religion to advance in harmony and even with mutual assistance, but I think it would be unduly optimistic to suppose that no further conflict between them will arise. When the religious experience is recognised as a genuine phenomenon, the scientific psychologist, inevitably seeking to correlate it with other phenomena, is most likely to bring it into relation with psychological states some of which are now regarded as abnormal or even pathological. It is scarcely to be expected that men with little scientific curiosity, but to whom their religion is the most precious thing in life, will view without protest what they cannot but regard as a degradation of the highest they know. It seems to me that this is the most likely ground of the conflict of the future. It will, of course, rest on a misunderstanding. Science has no concern with the intrinsic value of any experience; its object is only to find relations between one experience and another. The word 'pathological' has no significance for it—or at any rate not the chief significance that it has for those with a preconception of what is to be regarded as a healthy state. The scientist, for example, cannot be expected to overlook the fact that experiences that follow over-indulgence in certain liquors are likely to be related to experiences that follow under-indulgence, or fasting. That does not in the least mean that the intrinsic value of one experience may not be infinitely greater or less than that of the other. The vibrations of Caruso's vocal chords bore the closest possible scientific relation to those of Mr Frank Sinatra's—a relation which it would be folly to overlook—but the non-scientific music-lover is not likely to hail this discovery with delight. The highest wisdom lies in recognising both the relations and the very different values of the things related.

The discarding of an old prejudice and the cultivation of a new outlook are not matters that can be completed in a moment. One first catches a glimpse of a new way of regarding things, and begins to see a few outstanding features of his surroundings in a new light. But he does not immediately realise that the whole scene has been transformed. Deep-seated beliefs remain, incompatible with the new outlook though they may be, and only gradually begin to take on a strange appearance and arouse misgivings. For a long time, like Copernicus, to whom the heliocentric doctrine never succeeded in showing the unreality of the celestial spheres, he unconsciously maintains two inconsistent attitudes, and in all good faith harbours beliefs fundamentally at variance with one another. The Victorian presuppositions established themselves so firmly in the minds of our immediate ancestors that not even the example of the founders of modern science and the faithful observation of their principles by their successors can enable us yet wholly to escape the influence of the false notions. It must be left to the future fully to realise the emancipation that we can at least see to be inevitable.

HERBERT DINGLE

MINDLIKE BEHAVIOUR IN ARTEFACTS *

1 *Introduction*

In popular usage the term *machine* has come to stand for something which is essentially servile. The commonly-heard expostulation that ' a machine only does what you tell it to do ' thus becomes virtually a tautology. The term *artefact* in the sense of ' artificial construct ' has been used in the title of this paper in order to minimise misunderstanding from this cause, for the mechanisms with which we shall be concerned will differ radically from the common notion of a slave-machine.

Equally important is the fact that we shall not be concerned with existing *digital computing machines*. For certain functions the artefacts we consider may make use of digital techniques ; but present-day digital computers are deliberately designed to show as few as possible of the more human characteristics. Originality, independence in opinion, and the display of preferences and prejudices are not favoured by the mathematician in his computing tool. The comparison of *contemporary* calculating machines with human brains appears to have little merit, and has done much to befog the real issue, as to how far an artefact *could* in principle be made to show behaviour of the type which we normally regard as characteristic of a human mind.

Our enquiry might be roughly framed in the form of three questions marking progressive stages :

(i) Can an artefact be made to show the behavioural characteristics of an *organism ?*

(ii) How closely in principle could the behaviour of such an artificial organism parallel that of a human mind ?

(iii) On what philosophical issues, if any, do these possibilities have a bearing ?

2 *Goal-directed activity*

One of the most distinctive features of an organism is that of *goal-directed activity*. Associated with this feature are a group of concepts such as *purpose*, *control*, and the like, which cease to have

* Received 5·iii·51.

meaning in its absence. To avoid committing anthropomorphism—one of the two currently unforgivable sins—we may define the statement ' A seeks the goal X ' as follows :

> Let the current state of A (plus its environment) be defined as Y. Let X define that state of A-plus-environment which we term the *goal of A*. Then the statement above implies that the activity of A in a defined group of circumstances is such as *inter alia* to minimise the discrepancy between X and Y.

In a still more formal sense we may consider the two states X and Y to be representable by two points X and Y in an abstract space, the coordinates of each point being given by some set of parameters defining the corresponding state. We may then say that A functions in such a way as to minimise the interval XY, or some time-average of the magnitude of this interval. The situation may then be depicted in the manner of Fig. 1, which indicates the essential requirements for such goal-directed activity to be possible.

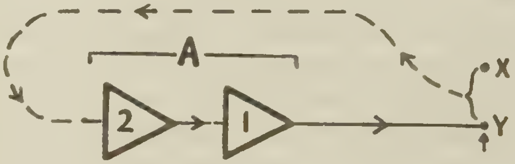


FIG. 1.

(i) There must be an effector (1) capable of altering the state Y. This perhaps would qualify for the title of ' machine ' in the usual sense.

(ii) The activity of this effector must be controllable by an element (2) capable of receiving information.

(iii) Information as to the magnitude of XY must be *fed back from the field of activity* to the controlling element (2), there to give rise (after analysis) to activity in (1) leading to the minimisation of some measure of XY.

There are many familiar examples of devices satisfying these requirements, ranging in complexity from the simple thermostat, in which the effector is a heating unit and the space of XY is a one-dimensional scale of temperature, to the self-directing missiles and automatic processing plants with multi-dimensional fields of activity which are now emerging from the blue-print stage. They are all characterised by the *circulation of information* indicated in Fig. 1,—by the presence, in other words, of *feedback*. Perhaps the simplest form of control is one in which the frequency or intensity of activity is proportional to the discrepancy XY. This evidently has a state of equilibrium only when the discrepancy is zero. (Such 'error-operated' fedback systems may, however, easily become unstable in the presence of delays or inertia in the information loop, so that the control element (2) must normally include quite complex computing operators if the information is to be used to optimum effect.)

The impression seems to be current that fedback systems form a class by themselves, subject to different laws from those of 'straight' systems. It is perhaps worth pointing out here that from the physical standpoint a fedback system is only a particular case of the general non-fedback type. If an input x to the control point of a normal mechanism causes an output y which is some function $F(x)$, then the introduction of feedback amounts to setting x equal to some function $G(y)$. The problem set by the system is thus the solution of the normal equation $z = G(F(x))$ for the special case wherein $z = x$. No exemption from ordinary laws accrues. The features of interest in its behaviour arise from the limitations on its behaviour-pattern set by this equation.

The systems we have considered can evidently be thought of from another standpoint. If we focus attention on the transformations of information which are usually necessary in (2), we can think of the latter as a type of computer, which makes logical deductions from data acquired from a field which is *affected by the results* of the deductions. This leads us to ask what types of field are open to the activity of such an artefact. Among the most interesting are the following:

(*a*) *The field of physical variables outside the artefact.* This we have already considered in the examples above.

(*b*) More interesting, the field of physical variables *widened to include the artefact itself.* In a primitive sense, an artefact capable of receiving and acting on information about the state of its own body can begin to parallel many of the modes of activity we associate with self-consciousness. At a lower but equally important level, as Ashby's 'Homeostat'[1] illustrates, it can adjust its internal modes of functioning so as to become in principle self-repairing.

(*c*) The field of *mathematical and other logical propositions.* The interest of this field from our viewpoint lies in the possibility of

[1] W. R. Ashby, *Electron. Eng.*, 1948, **20**, 379

generating continuous and significant trains of reasoning, not dependent on the continuous supply of information by an operator. To a limited extent this mode of operation is realised in present-day computing machines ; and the well-known discipline of Boolean algebra enables logical deduction also to be arithmetised for treatment, by the same method as numbers on the scale of (2).

(*d*) In any or all the above, a field *including a human interlocutor*. This is perhaps of greatest interest. An artefact can for example be designed to play a game of chess with a reasonable frequency of success against a human opponent.[1] In principle such an artefact may truly be said to engage in logical *dialogue*, and no barrier of principle prevents the field of dialogue from covering topics other than chess.

But we may cut short the enumeration of these possibilities, for they have already received much publicity. Among the more important functions of which we shall hereafter assume an artefact to be capable, we may list the following in summary :

(*a*) *Receiving, selecting, storing, and sending information.*

(*b*) *Reacting to changes in its ' universe,'* including *messages* and *data on its own state.*

(*c*) *Reasoning* deductively from premisses which can include the results of previous deductions and data on the relative success of different courses.

(*d*) Observing and controlling its own activity, whether symbolic or otherwise, so as to further some *goal*. This may be only a very general objective, such as the maximisation of efficiency in some defined sense, or the attainment of equilibrium, leaving a wide scope for the development of subsidiary ' purposes ' *ad hoc* by the artefact as the result of its experience.

(*e*) *Changing* its own pattern of behaviour as a result of experience so as to develop quite complex and superficially unpredictable characteristics capable of rational description in purposive terms.

The above enumeration has not been made in the belief that these possibilities are philosophically significant, except in so far as they remove confusion due to insufficient thought. For they are merely consequences of a principle which is generally accepted : any pattern of observable behaviour which can be specified in terms of unique and precisely-definable reactions to precisely-definable situations can in principle be imitated mechanically. (As a last resort, the decomposition of the specification in terms of Boolean algebra can always in principle lead to a description of a relay system which satisfies it.)

---

[1] C. E. Shannon, *Phil. Mag.*, 1950, 41, 256

It is suggested in other words that no new philosophical issues are raised by the possibilities so far considered. These artefacts are merely deterministic performers of functions which we ourselves would describe as ' mechanical ' were we to carry them out. There seems no reason to deny, however, that in principle such artefacts could merit the title of organisms.

3 *A probabilistic reasoning-mechanism*

The type of artefact that we have just considered would meet many of the classical tests for mindlike behaviour. We have still, however, to meet the question implicit in our first paragraphs : Is it possible for an artefact to show originality, independence of opinion, and, for example, such illogical human characteristics as prejudice, and preference ? Or can we find analogues of such impalpable processes as the weighing of evidence ? In short, how closely in principle could the behaviour of an artefact parallel that of the human mind ?

To answer this question we shall consider a rather different type of artefact, which is perhaps most easily introduced as a development of the first. We have been making an implicit assumption that all the data used are exact, and lead to deductions which are unique and certain. This follows from our assumption that decomposition into elementary ' yes-or-no ' propositions is possible without distortion. But in human intercourse this is seldom the case. Data are only moderately certain, and the closest attainable approach to a unique conclusion is often an estimate of the relative probabilities of several. It appears likely that an artefact to imitate the more human thought-processes, should operate on a much less deterministic and inflexible basis. What is required in fact is a means of introducing the concept of *partial truth* or *probability*.

This is a twofold problem. First, we require means of handling, combining and computing probabilities—in effect, of making statistical analyses. Secondly, we should evidently design our artefact so that its actions based on such probabilistic data can parallel those of a rational mind in the same circumstances. This implies that the *statistical structure* of its behaviour pattern shall be that logically justified by the statistics of the data if it is to be described as reasonable and unprejudiced. It is possible to envisage a very simple mechanism which can meet these requirements admirably, and incidentally has a well-known neurological parallel. It is the mechanism of *threshold control.* Consider an element which changes from one to another of two states when a controlling input exceeds a certain threshold level. If the magnitude of signal and threshold are precisely specified, the consequent state of the element is uniquely determined (excluding the infinitely improbable case where the magnitudes are exactly equal). But if now we introduce an element of randomness—which is inevitable in any real case—we shall only be able to say that the probability of change of state is some known function of the signal and threshold magnitudes. For example, we might use electronically generated random noise-signals to perturb the threshold level, in such a way that the individual actions of the element would become unpredictable *in principle.* At the same time its *statistical* behaviour pattern would reflect quite precisely the information-content of the threshold : signal configuration. In short, such an element would behave as if *uncertain* of the truth of the proposition represented by the stimulating signal.

It is not of course necessary to adhere literally to the notion of a determined threshold randomly perturbed. Any functional perturbation which can give meaning to the notion of *control of transition-probability* is sufficient to provide the kind of statistically-sensitive element we require. In an artefact constructed on this new principle, information would be represented not only by the current excitation-patterns, but also by the changing probabilities of excitation. In fact it could be thought of as two interlocked computing networks, one representing and handling information in the form of discrete symbols, the other handling information relative to the probabilities to be associated with them.[1]

At the risk of oversimplification, we might clarify the picture by describing our earlier reasoning-mechanism as a switchboard with rigidly-interlocked levers. We have now moved from this notion to the notion of a switchboard in interaction with an analogical network controlling the *probabilities* of throwing the switches.

---

[1] This complementary relationship is closely analogous to that between structural and metrical information in the formalism of information theory (D. M. MacKay, *Phil. Mag.*, 1950, 41, 289), which in fact was the stimulus leading to the ideas now presented. The connection will not be developed here, though it may be remarked that the representation of *meaning* by the orientation of an information vector suggests a direct interpretation of the concept in terms of the functioning of a statistical mechanism of this kind.

We have in fact passed by easy stages to a new concept of a reasoning mechanism, in which the control of transition probabilities plays a dominant rôle. ' Thinking ' in such a mechanism becomes a stochastic process, proceeding along paths determined only statistically. Guiding the statistical process is the network which computes and governs transition-probabilities, which may in many cases be more important than the excitation-pattern.

It is natural to ask what it is which prevents such activity from becoming nonsensical. In the first place, it must be remembered that only an increase in threshold is necessary to make the mechanism operate on the simple digital principle. It is thus capable of all the activities of the digital artefact considered earlier. Lowering of threshold need take place only where statistical ' thinking ' is appropriate. But in the second place, the activity is always distinguished from nonsense by its statistical pattern—by the effects of the guiding transition-probabilities at each point of divergence. Individually its actions may be unpredictable ; statistically (or collectively) they make sense. The behaviour pattern of a human being in many respects provides a parallel here ; and there is no reason in principle why precisely the same degree of reasonableness should not be attained by the artefact, since its upper bound of performance is the perfection of rigid logic.

It is not difficult to see how such a flexible mechanism can manifest most of the ' typically human ' characteristics. Continuously-variable (and irregularly excitable) *prejudices*, *preferences*, and other ' emotional ' effects can obviously be shown if transition-probabilities are linked appropriately to the ' causes ' of the prejudices. *Weighing* of evidence can be represented by subliminal manipulation of probability-amplitudes. *Originality* of a kind can be constantly in evidence because of the lack of a deterministic link between input and output ; and on those occasions when a random excitation-pattern finds itself in logical equilibrium with the current data-pattern, quite significant contributions could be made to a train of thought, with full claim to be called original.[1] *Learning* in such artefact could take place in more than one way. Discrete representations similar to those used in digital computers could of course be used. But the most natural process would be one in which the frequency of past success or failure of a given action determined the transition-probability to that action in future. Such a self-guiding learning mechanism has been built by the author. It is readily envisaged both in electronic and physiological terms, and has the advantage that stored information is immediately available at the relevant point. It seems increasingly likely, in view of physiological evidence, that the human brain also may retain much of its information in this form rather than in localised ' stores.' In comparing the possibilities of such an artefact with the performance of a human mind, it is important to remember the part played in the development of the human mind by intercourse with other human beings. We must be prepared to imagine as much care and attention to be spent on the ' education ' of our artefact as on that of a child. If we do so, it is difficult to deny that such an artefact could be made to develop statistically consistent modes of behaviour which in a human being we should describe as constituting a personality.

---

[1] Normally, of course, the life of a sequence uncorrelated with the main stream of thought would be short, because of the improbability of the necessary transitions.

Our conclusion in fact may reasonably be expressed in a generalisation of our previous axiom, as follows : ' Any pattern of observable behaviour which can be defined *statistically*, in terms of *probable* reactions to given situations, can in principle be shown by an artefact.'

The implications of this conclusion are of greater interest.

4 *Abstraction*

It is not the purpose of this paper to enter into technical details of artificial mechanisms. The foregoing sketch has sought only to establish the width of the class of functions which can be realised in an artefact, so as to place the later discussion in proper perspective. There is, however, one central problem which deserves closer consideration because of its connection with longstanding philosophical issues. It is the problem of abstraction—the recognition of pattern or *Gestalt*. Related to it is the question whether an artefact could with reasonable frequency generate significant hypotheses.

We have indeed begged a thorny question in assuming implicitly that all incoming data were *intelligible* to our artefact. How is it possible to ensure that the significant invariant of a pattern of data is selected for propositional representation ? Given a situation in which recognition of *triangularity* is required—to take a stock example—how can we ensure that the response of the artefact could remain invariant with respect to changes in size, shape and orientation of a triangle ?

It is a fact which may perhaps be thought surprising, that contemporary artefacts can much more readily be made to perform elaborate calculations that would strain a human brain to the limit, than simple feats of recognition which children or some lower animals perform instinctively.

Two fundamentally distinct approaches to this problem are possible. The first may be called the passive or template-fitting method, which has been described by McCulloch [1] and Wiener [2] among others. A typical sample of the pattern to be recognised is stored in the artefact as a kind of template. Incoming signals are now subjected to a systematic series of transformations inverse to those under which the pattern is invariant. If the artefact is to recognise a triangle irrespective of its size, shape or orientation, it must change all three aspects of the incoming signal systematically until it matches the stored sample-signal—the ' ideal triangle '—representing the concept ' triangularity.' At this stage the artefact can adopt a course of action indicative of its *recognition* that the pattern is triangular.

Such an artefact recognises in the act of *reception*—hence our use of the term ' passive ' to describe the mechanism, which acts essentially as a filter. Some features of the structure of the visual cortex have been adduced as evidence in support of the hypothesis that cerebral pattern-perception functions in the same manner,[3] but the idea does not appear to have found universal favour ; and for our purpose it is possible to envisage a mechanism on a different principle, which is not open to the same objections.

By way of introduction to this second notion, let us first consider the way in which a blindfold man might seek to recognise a solid triangular figure, by moving his finger around the outline.[4]

He requires essentially to perform two kinds of motion—rectilinear movement, and sudden changes of direction. As his finger moves round the outline, he finds it necessary to issue to them just two types of order, in a characteristic sequence. This sequence is in principle invariant with respect to the size, shape or orientation of the

[1] W S. McCulloch and W. Pitts, (1) *Bull. Math. Biophys.*, 1943, **5**, 115-33 ; (2). ibid. 1947, **9**, 127-47

[2] N. Wiener, *Cybernetics*, Wiley, 1948

[3] W. S. McCulloch and W. Pitts, *Bull. Math. Biophys.*, 1947, **9**, 134-36

[4] The usefulness of defining the form of a geometrical figure by its ' natural equation ' (i.e. the equation specifying curvature as a function of distance along the periphery) has been noticed independently by a number of workers. Some development of the idea has been found in the writings of Craik (K. G. W. Craik, *The Nature of Explanation*, Cambridge, 1943) ; and Dr. O. Straus of Massachusetts Institute of Technology in a personal communication (25.1.51) has described a method of identifying curvilinear figures on this principle by computing their natural equation.

triangle. To the blindfold man, the concept of triangularity is invariably related with and can be defined by the sequence of elementary responses necessary in the act of replicating the outline of the triangle.

Let us generalise this approach to the problem of recognition, and consider now an artefact whose response to incoming stimuli of any kind is an *act of replication*, in some formal sense, of the stimuli received. In other words, disequilibrium signals are generated in the artefact and cause activity therein until there is a sufficient degree of resemblance between a synthetic replica and the incoming pattern. This situation is simply a particular case of the general scheme of Fig. 1, wherein X and Y would now represent the incoming and imitative patterns respectively.

At this point we introduce a crucial feature. We suppose that our artefact possesses at any time a finite number of standard methods of replication—a finite number of commands and command-sequences to the replicating effectors. Evidently then, if recognition is a function of the act of replication, the total of recognised experience for the artefact is describable and identifiable in terms of this one set of commands. *The elementary acts of replication define the basic vocabulary in terms of which the artefact describes its own experience.*

The term ' experience ' though anthropomorphic appears to have justifiable metaphorical use here, since it refers to that fraction of the total of received stimuli which has evoked the analogue of *conscious awareness* in the artefact—the fraction with which subsequent behaviour may show correlation.

Evidently the problem of recognising complex patterns now reduces to the problem of learning to make complex (formal) replicas, which is of the same class as other problems of coordination such as learning to walk or write. And since our artefact uses as its elementary symbols the elementary acts of replication, no problems of *identification* arise. Complex concepts are represented by complexes of symbolic (internal) acts of representation.[1] The significance or meaning of any concept to the artefact, is *described* in its language by the same complex of responses as that which makes up its name. This introduces a vast simplification into the kind of learning-mechanism which we may envisage. Transition-probabilities now require to be established, not between a stimulus and an action via an identification-link, but between one action and another—between the act of replication constituting recognition and the learned response which should follow recognition.

---

[1] It should be clear that the input dealt with by the replicatory mechanism is generally in quite a different physical form from the original input to the sensory receptors. An olfactory stimulus for example might be mapped electrically, and would not have to be replicated by generating odours !

In the above discussion we have incurred two debts. We have omitted all mention of the statistical aspects of recognition ; and we have tacitly assumed that our artefact has been taught to make the standard sets of responses which symbolise the universals of its world. It is almost time to leave technical discussion for more philosophical stock-taking, but a very brief indication of solvency may be given.

The first debt is readily discharged. All that has been said earlier about statistical thought-sequences can apply equally to the process of recognition. Replication is, of course, never perfect. A practical recognitive artefact need only have a finite resolving-power in its comparator, and could use parallel-operation to enable it to choose what it estimates to be the *most probable* replicatory command-sequence to ' describe ' a given pattern. This has important implications for the concept of *meaning*. Evidently the meaning of a symbol can here be something more general than the specification of a unique combination of ' eigensymbols.' To our probabilistic artefact, a symbol means a probability-distribution over the set of eigensymbols ; the selective operation performed by the symbol is a statistical one and may perhaps be validly followed by any one of several different responses. It may equally be impossible to give a unique verbal definition even in terms of responses, of some concepts appearing in the thought-process of the artefact, since these may be defined only in terms of the statistical conjunction of several responses.

Our second debt is a more important one. Could an artefact discover and recognise new invariants independently of prior instruction ? Again a statistical mechanism makes it easy to envisage such a process. The only pre-requisite is a sufficiently frequent recurrence of the invariant in the patterns which are replicated. For in the absence of a ' recognised ' pattern, the artefact could be made to try automatically in random fashion, various sequences of replicatory orders. The relative success of each could be caused to influence the probability of future trial. Thus if one of these happens to correspond to a recurrent pattern, it will rapidly grow in statistical status. If the recurrence of the response-pattern is persistent enough for it to become associated with actions, the pattern enters the vocabulary of the artefact as a new ‘ universal.’

In a final word we might examine the relation of this last mechanism to the making of hypotheses. A hypothesis in this context is a symbolisation not evoked as a direct replica of received stimuli. It is essentially a response-train prepared in advance of an expected pattern of stimuli. The simplest hypothesis—that past experience will recur—is thus made automatically by our artefact. But in the face of contradictory subsequent experience, it can be designed to adopt procedure identical in principle with the exploratory speculation which forms at least a large component of what *we* understand by making hypotheses. And because the transition-probabilities can be made to change automatically in accordance with the results of trials, there is no obvious reason in principle why the process should not converge as frequently and as fruitfully as in human speculative thought. Indeed it is a tenable hypothesis that the human cerebral mechanism may operate in just such a manner.

We may generalise our concept in a further step. One way in which our artefact could respond, is to alter the order-pattern (or the relative probabilities of orders) in a standard group of orders. This is an important form of response, for it corresponds to the making of an *abstract* hypothesis—an hypothesis concerning an abstraction, as distinct from one concerning a pattern of stimuli. In other words, the response of an artefact may be to *alter the mechanism controlling response*. This is equivalent to recognising the pattern formed by the response-orders. Here we have the first step in a *hierarchy of abstraction*, in which the patterns formed by the responses of each level become the subject matter of the response-vocabulary of the next higher. This possibility may repay more detailed study on another occasion.

In summary we may note some of the more important features of the type of artefact we have been developing.

(*a*) We take for granted the ability to parallel the more mechanical aspects of human behaviour discussed in earlier sections.

(*b*) The distinction between *reception* and *perception* is fundamental to its operation, the latter being characterised by the element of *response*.

(*c*) The points (or regions) of *conscious attention* in a field of received data have their analogues in the regions under *active replication*.

(*d*) Many familiar psychological phenomena, such as optical illusions, find direct analogues in the normal functioning of the replicatory mechanism.

(*e*) The *basic symbols* of the vocabulary of the artefact are the *elementary acts of response.*

(*f*) The *universals* of its field of discourse are those *sequences of responses* which have found repeated application.

(*g*) Such an artefact is automatically adapted to concentrate on the *changing* features of its environment, since it is these which require the issue of new commands to the replicatory mechanism ; and by (*e*) above this automatically amounts to *naming* the new features.

(*h*) It is possible to find an analogue for the activity of *making hypotheses*, which seems hard to distinguish in principle from the human parallel.

(*i*) The element of indeterminacy in the *modus operandi* is fundamental to its proper function in a statistically-fluctuating universe.

5 *Philosophical Stock-taking*

On what philosophical issues do these possibilities have a bearing ? A systematic study of their implications would be lengthy ; but they appear to illuminate a number of concepts from a new angle.

The interesting aspects of the behaviour of our artefact arise principally from the combination of two features—its goal-directed activity and its ' reasonable indeterminacy.' It has already been suggested that the first of these raises no new philosophical issue. It does, however, have some purgative influence on our thinking about the concept of *mind.* Mindlike behaviour is here conditional on the *circulation of information* in the sense of Section 2, for the concepts of mindlike behaviour are defined only for closed-loop circulatory systems.

It now becomes easy to see the fallacy inherent equally in the analytical approach of the classical mechanist and in the Cartesian compromise. To claim that analysis of the nervous system into parts reveals ' no sign of the soul ' is as indicative of a false approach as to suggest that any one organ might be the seat thereof. In terms of the information-diagram, the position is directly analogous to that of a man seeking the ' residence of triangularity ' among the individual dots of a triangular dot-pattern.

We have in fact an ideal example of the destructive effect of abstracting a system from its environment, when the concepts of interest are essentially properties of the system-plus-environment.

A more subtle fallacy, however, lies in wait. The temptation is now to say that the mindlike features are ' nothing but ' abstractions from the physical description. The question-begging phrase ' nothing but ' appears to have no justification. The concepts relating to mindlike behaviour form a different logical group from those which appear in the analysis of elements of the artefact, so that it is true that the corresponding descriptions may be logically immiscible for some purposes of deduction. But for our recognitive artefact at least, the abstractions have as much logical respectability as the data in which they are recognised, since all are exemplified by replication-operations of one kind or another. All, in short, have operational status, at one level or another.

The false dualism which used to be expressed in the question ' how can matter produce mind ' would now seem to have its origin in a genuine dualism of conceptual frames of reference, defined respectively for the viewpoint of actor and spectator. The situation is not a symmetrical one, but the concept of complementarity whose value we have been led to recognise in physics appears to have an analogue here that would repay development. The dualism of wave and particle in physics is resolved neither by arbitrary denials of ' reality ' nor by ' explanations ' of one as ' nothing but ' an aspect of the other. The process of description is seen as a selective or projective operation ; and it is not so much the validity but the appropriateness of a description which requires to be discussed in any given situation. Paradoxes arise when concepts defined for one logical background are mixed carelessly with those defined for another. Descriptions in terms only of one group or the other may both be valid. It is not the descriptions which are exclusive, but the logical backgrounds in terms of which they have meaning. The moral is obvious, and seems to admit of large-scale transfer to other fields of thought.

The artefact is also of interest in providing a direct analogue of the concept of *consciousness*. Consciousness, behaviourally speaking, is represented as we have seen by the region of the field of data evoking active response by (formal) replication. *Self*-consciousness appears to be shown if the field of replication includes a representation of the artefact itself, which of course introduces no special difficulty. The same situation could automatically ensure that the artefact would act as if aware that external events were located in a world external to its own body.

The application of a similar interpretation to the concept of human consciousness is of course purely hypothetical, but the parallel is at least suggestive. It would imply, if valid, that 'I' am the totality of currently reacting elements of my body, which are organised in terms of information-linkages so as to constitute an organism. The continuity which we predicate of our perceived experience despite the discontinuities of our received data, would find its origin in the persistency of the command-pattern by which experience is symbolically replicated and *ipso facto* perceived and described. Concepts such as responsibility are group-concepts defined for the totality, and define a valid calculus of behaviour at the level and in the language of *conscious* activity. Along such lines it seems possible that a consistent probabilistic theory of personality might be developed which could find a complementary place for some of the psychological dicta currently debated as antagonistic, particularly in relation to pathological conditions.

Another concept which now appears in a new light is the notion of randomness. Information theory enables us to give precise significance to a definition of randomness as 'lack of information-content.' In particular, a completely random sequence *to a given receiver* is definable as one which exercises no selective operation on the information-space of the receiver. (*Absolute* randomness has probably as little meaning as other notions defined by prefixing the word 'absolute' to operationally defined concepts.)

We are accustomed to think of completely random activity as meaningless and dull. But our artefact shows randomness in the domain which in a human being is that of *free will*; and behaviourally there is no reason in principle why the two should be distinguishable. All *systematic* components of human behaviour-patterns can in principle be simulated. What remains is by definition devoid of systematic content for the observer, i.e. it exercises no selective action on the information-space of the observer. Equally by definition, it must then be classified as the 'completely random component' of the human behaviour pattern *from the observer's point of view*. Yet in the context of the systematic component it admits of a reasonable interpretation as the exercise of free choice; and by the actor himself the calculus of responsibility is normally acknowledged to be directly applicable to it as such.

The point cannot be pressed in detail here, but it may bear further consideration, since we are so often inclined to use 'mere randomness' as a term of dismissal or almost of explanation.

The connection between the apparent free will of our artefact and the determinate components of its behaviour-pattern inevitably suggests an analogy with the hoary problem of human free will. This is usually epitomised in the question, 'Is human free will genuine ?' The question in this form is imprecise, but the parallel may be illuminating. The choices of a free man are seldom devoid of a statistically-predictable component ; someone who knows him well can usually score a significant frequency of success in predicting his choices, though it is most unlikely that they form a stationary time-series. In the same way the choices of the artefact, given a knowledge of the various threshold levels defining transition-probabilities, are statistically predictable on a short term basis. The suggestion is that the choices of a free man may likewise be governed by statistical distribution-functions which have a physiological representation and are in principle determinate ; but that individual choices can be unpredictable in principle, and it is probable that the distribution-functions are indeterminable in practice.

To complete a sketchy stock-taking, it is suggested that these developments raise no new *theological* issues. This is, of course, a personal view ; but the so-called 'theological objection' against which Turing [1] tilts in a recent paper appears to be a windmill without an owner among reputable contemporary theologians. Doubtless a full realisation of foregoing implications will have a purgative action on unwarranted speculative accretions in theology, but the central concern of Christian theology at least is framed in terms of the calculus of responsibility, whose categories in the author's view are unaffected by changes in the complementary categories of physical process used to describe brain function. No reputable theologian expects to find physical laws disobeyed in the human brain ; and it is difficult to see how elucidation of the particular physical processes which happen to be used can in any way be relevant to the claims (whether admitted or not) which are made by Christianity on the personality whose thought is mediated by those processes.

6 *Conclusions*

Our principal conclusion is that we have failed to find any distinction in principle between the observable behaviour of a human brain and the behaviour possible in a suitably designed artefact. We would do well, however, to retain a sense of proportion ; the human central nervous system is estimated to comprise some 10^{10} elements, each of considerable complexity. It is unnecessary to point out the practical difficulties of constructing, supplying and maintaining an artefact of comparable complexity, and the possibility need not be taken seriously. At the same time it must be realised that a much smaller number of elements would provide a complexity of behaviour comparable in most respects with that of human beings, so that no deductions in either direction need be made dogmatically at this stage.

---

[1] A. M. Turing, *Mind*, 1950, **59**, 433

We have seen that the designing of an artefact on statistical principles, using a combination of quantal and continuous processes, enables its statistical behaviour-pattern to be intelligible despite a controllable measure of freedom in individual choices. In such an artefact analogues of concepts such as emotion, judgment, originality, consciousness, and self-consciousness appear. It is capable of performing abstractions, and generating significant hypotheses with reasonable frequency—including hypotheses about its own mechanism for generating hypotheses.

Quite unintentionally, this paper appears to have provided an answer to some strictures of Professor Popper on the capabilities of 'machines.'[1] Professor Popper's emphasis on the limitations of Science (with a capital S) and on the dignity of human personality, we should warmly endorse. His remarks that 'calculators' can have only 'senseless powers of producing truths,' and 'possess no method of picking out the interesting or important ones' are certainly true of contemporary computers, for good reasons. But when he says that 'It is only the human brain which can create interests, purposes, problems and ends'—then in face of the foregoing possibilities, he appears to shoulder a considerable *onus probandi* for no very clear reason.

This said, we might perhaps go even further than Professor Popper in insisting that the elucidation of the actual mechanisms mediating human thought, or the development of artificial organisms capable of a comparable function, affects neither the status of man's thought nor the extent to which he is answerable for the choices made on the level of moral and spiritual responsibility.

D. M. MacKay

---

[1] *The British Journal for the Philosophy of Science*, 1950, **1**, 194-5, especially footnote on p. 195

THE RELATION BETWEEN THE TIME OF PSYCHOLOGY AND THE TIME OF PHYSICS

PART I*

Summary

THIS paper seeks to elucidate the phenomenon known in psychology as 'the specious present,' by postulating a two-dimensional theory of the extensional aspects of time. On this theory, the usual logical and psychological difficulties, encountered in current accounts of this phenomenon, can be resolved. For, when there are two dimensions of time, the same event may be without extension in one of these dimensions ('transition-time'), while it is nevertheless finitely extended in the other of these dimensions ('phase-time'); so that in a definable sense the phases of a finitely enduring event, though successive in one time-order, yet are contemporary in the other.

The epistemological standpoint implicit in the paper is generally similar to the one Bertrand Russell has put forward, in his *Physics and Experience* (Cambridge, 1946), and his *Human Knowledge: Its Scope and Limits* (London, 1948) (allowing for the changes which would be required in Russell's theory to take into account a second time dimension). A 'psycho-neural parallelism,' or one-one correspondence, is postulated between features of certain 'experiential events' (namely, those experiential events normally held to be happening to some person's *mind*, which are describable in the language of psychology); and features of certain 'physical events' (namely, those events described in the language of physics, chemistry and physiology, which are ordinarily conceived as happenings in that same person's *body*). These physical events are conceived of as being causally connected with events in the physical world outside the experient's body, by means of the concepts of light waves, sound waves, chemical stimuli, and consequential processes in the nervous system (central and peripheral) and sense-organs, in the usual way.

In terms of this psycho-neural parallelism the physical correlate of the finite temporal span of the specious, or experiential present, is to be found in certain consequences of the uncertainty principle in quantum physics; according to which there is a finite interval of time necessarily associated with a nearly precise determination of energy levels, and of transitions between them. Some of the physical implications of this theory, applied to

---

* Received 13. iii. 51

processes in the material structure of the human body, are discussed qualitatively. But, for the reasons given in the last section of the paper, a quantitative treatment is not yet possible. The paper is greatly indebted in regard to the physical application of the two-dimensional theory of time, to the discussion of the pentadic group structure in Eddington's *Fundamental Theory* (Cambridge, 1946 and 1950) ; and in particular to the treatment there of the phase-variable as the ' time-analogue ' in the quantum statistics of stationary states.

1 *The Phenomenon of the Specious Present*

The phenomenon known in psychology as ' the specious present ' was first clearly recognised by the great American psychologist William James. But since James first wrote of it there has been much discussion thereon by British psychologists, from whom the following short descriptive quotations are taken. Thus, Dr William Brown, formerly Director of the Institute of Experimental Psychology at Oxford, writes :

> We, as conscious individuals, are aware not only of the immediate present—that is of a point of time—but also of the immediate past and immediate future, in the same moment of experience ; we are conscious of what is called the Specious Present, and the Specious Present, or the practically cognised Present is, to quote William James, 'no knife-edge, but a saddle-back with a certain width of its own.'[1]

The essence of the phenomenon is that, within one momentary experience, or 'complete complex of compresence,' as Bertrand Russell has described it in his *Human Knowledge : Its Scope and Limits*[2] (for instance pp. 37, 312-322), we find the presentation of data which are, in one way, indubitably successive in time ; yet also, in another way, they are equally indubitably contemporary in that they are 'compresent together,' in that one unitary 'moment of experience.' Any number of examples of this could be given ; but I hope that, in the interest of concision, the following quotations, taken from an eminent British psychologist, the late Professor G. F. Stout, will suffice to emphasise the main point of difficulty. I quote from his *Studies in Psychology and Philosophy*[3], pp. 169-170, where he says :

> What is the Specious Present ? If I seek an answer from those who have written upon the subject I find only two points clearly expressed : (1) In the Specious Present there somehow really is a finite time, in which a later experience succeeds an earlier. (2) That nevertheless

---

[1] *Personality and Religion*, London, 1946, p. 37

[2] London, 1948

[3] London, 1930

> . . . it is . . . as if the whole were present together. If we push inquiry further we get no precise and consistent answer from those who have made much of the Specious Present. They simply leave us with the paradox that the subject both does and does not apprehend change and succession in his experience.

Stout gives an illustration of the difficulties inherent in the way of understanding such a conception by describing the situation, familiar to British readers, of hearing the characteristic English postman's 'rat-tat,' executed upon the door of a house, to signalise the delivery of mail. He says :

> There is successiveness : the first knock is heard before the second. There is also retentiveness : the individual's experience, when the second knock occurs, has a character which it would not have if he had not heard the first. Further there is *no reminiscence*. In actually experiencing the second sound the subject does not definitely discriminate it as a present occurrence from the first as a past occurrence. The second treads too closely on the heels of the first to admit of such discrimination between past and present as such. So there is no remembrance of the past as such. This just emerges subsequently when the whole experience of having heard the double knock is remembered.[1]

However paradoxical such a situation may seem on the current view of time, which treats it as being in its extensional aspect—the aspect capable of representation in a graph—one dimensional, there is no doubt that Stout's description fits the facts, both of everyday experience, and of scientifically controlled observation in psychology. Moreover, the fact that we continually experience contemporaneously, within the bounds of one specious present, discriminable features which are manifestly and indubitably ' successive,' in the only one dimension of time currently recognised in psychology and physics, is certainly a necessary condition for the *direct* awareness of movement and change. As Bertrand Russell says, ' When I see a rapid movement, I am aware that one part of the movement is earlier than another, in spite of the whole being comprised within one specious present. If I were not aware of this, I should not know whether the movement had been from A to B or from B to A, or even that changes had occurred.'[2]

The essential point recognised in these quotations, which are equally applicable to typical everyday experiences and to psychological laboratory conditions, is that, within a single specious present, there may be event-components which, in one way, are manifestly successive ; yet which are, in another way, equally manifestly, all compresent together as a whole. So that the many different phases of an event which are experienced as being manifestly successive, are yet all presented together at the same moment of time to an experiencer. In other words, in a speciously present experience, manifestly successive event-components can also nonetheless manifestly be contemporary in time.

---

[1] London, 1930, p. 170

[2] *Human Knowledge*, p. 226

Such manifestly contemporary and yet equally manifestly successive components are, I repeat, actually experienced together, here and now all at once ; the earlier with the later. It is not the case that the latest phase only is experienced as present, while the earlier ones are just remembered (as Stout said : 'There is no reminiscence'). Yet the situation has the sort of unity and simultaneity which led Bertrand Russell to assert : 'What I call a total momentary experience, has all the formal properties required of an " instant " in my biography. And it will be found that, where there is only matter, the " complete complex of compresence " may serve to define an instant of Einsteinian local time, or to define a " point-instant " in cosmic space-time.'[1] And again : 'A complete complex of compresence counts as a space-time point-instant.'[2] As will be clear from my subsequent discussion, I agree with Russell as to the *punctiformity* or point-like character of a 'total momentary experience,' in respect of *one* of the *two* dimensions I recognise in the extensional aspect of time ; but it will be equally plain that I do *not* agree with Russell's statement : 'What I call a " total momentary experience " has *all* the formal properties required of an " instant." '[3] And my reason for this is just the existence of the facts which I have been discussing in this section : that a 'complete complex of compresence,' or 'specious present,' comprises always a set of event-components which are not wholly punctiform, but are finitely extended in some dimension of time, their various phases being temporally connected by the relation of 'precedence.' Whereas in science an 'instant' has to be as wholly punctiform or without extension in *any space* or *time* dimension, as is a 'point' of elementary Euclidean geometry.

2 *The Solution of the Specious Present Paradox by the Postulation of a Second Dimension in the Extensional Aspect of Time*

I shall begin this section with some descriptions of verbal usage to help make the meaning of my theory clear. I start with a general description of how I am going to use the words 'experiential event'

[1] *Human Knowledge*, p. 37 [2] Ibid. p. 322 [3] Ibid. p. 37

and ' physical event ' ; for the concepts expressed by these words are fundamental to the understanding of my theory of two dimensional time, in relation both to psychology and physics. I then go on to describe my usage of certain more specific phrases, which express key concepts in my two-dimensional theory of time. Then I apply the theory expressed by these concepts to the elucidation of the paradox of the phenomenon of the specious present, which has already been described in the first section of this paper.

By an ' event ' in general I mean the characterisation of a region of space-time by one or more qualities or relational properties ; in other words, an event is the occurrence, in a certain spatio-temporal nexus of instances of a complex of characteristics. (These characteristics may be of various kinds. Thus they may be sensory, emotional, or physical ; qualitative, quantitative, or both.) As to what criteria are to be adopted to decide what should count as ' one event,' rather than a succession of events, the question is a difficult one. In actual practice the criterion generally adopted is the subjective one of the direction of the experiencer's interests. For example, a poet would probably regard the reading of a sonnet as *one* event, whereas a printer's type-setter would probably take the reading of the same sonnet as a succession of events, the boundaries of which were indicated by those punctuation marks known in America as ' periods,' and in England as ' full stops.' But on any view there is a sense in which a ' point event ' (i.e. a temporarily punctiform occurrence) must necessarily be counted as ' one event.' The characteristics, whose occurrence in a spatio-temporal nexus constitutes the content of what we call an event, can be either :

(I) *directly experienceable :* If so, the occurrence is an instance of what I mean by an ' experiential event.' We have already considered an example of this kind of event, in the first section of this paper, where we quoted Stout's account of the hearing (which is one specific form of direct experience), of sensations of sound, manifesting the directly sensible qualitative characteristics that are onomatopoeically indicated by the words ' rat-tat.'

Or, (II) the event may involve an occurrence of characteristics *not directly experienceable* in the sense just mentioned in (I) above. But nonetheless its occurrence can be inferred with varying degrees of probability by logical processes of construction and hypothesis, and by inferences therefrom. In an obvious sense what may be regarded as a single objective process may have *both* directly experienceable aspects and also others not thus directly experienceable. (The latter being instances of physical events : these physical event aspects include of course the descriptions scientists give of the *causes* of our having particular experiential events, these descriptions being expressed in terms of such logical constructions as molecules, atoms, electrons, photons, nerve cells, etc.)

The example I shall generally take is the one of ‘ seeing flashes of lightning,’ as we should ordinarily say. One reason for this choice is that recently there has been much careful scientific analysis of the phenomenology of such occurrences.[1] Moreover this example has two distinguishable aspects, one of which typifies what I have called an ‘ experiential event,’ since (i) the characteristics of visible brightness and whiteness, which characterise the spatio-temporal nexus of the flashes as I see them, are *directly experienced ;* (ii) these visible characteristics stand in a directly apprehended spatial relationship to other components of my visual field, having immediately visible boundaries and specific ‘ form-quality ’ or ‘ Gestalt ’ ; (iii) they occur within a spatio-temporal nexus, which relates the extensional aspects of their occurrence to components of my present visual field by spatial relations (such as are expressed by the words ‘ above,’ ‘ below,’ ‘ beyond,’ etc.) ; and to components of past and future sense-fields, which can be ordered in terms of the temporal relations of ‘ earlier than ’ or ‘ later than.’ The other aspect of such objective process typifies what we call a ‘ physical event,’ which would be described by the physicist in terms of such logical constructions as transitions between energy-levels in cloud-molecules, electric discharge, emission of photons, etc., all of which would be regarded as *causes* of my seeing the flash.

Common sense is not sufficiently interested in careful epistemological analysis to make the distinction we have drawn between the status of experiential events and physical events. Moreover at the level of commonsense thought there is little point in this distinction, or in that between the notions of ‘ object ’ and ‘ cause.’ At the more complex and elaborate level of the various sciences (especially physics) the underlying inferred physical events are *postulated* only as *causes*, or as independent variables in a functional relationship.

Common sense does, however, recognise another distinction which has some connection with this, namely, that the nature of a *physical* event can, in a sense, be said to be ' public ' ; (whereas, the characteristics manifested in *experiential* events are ' private ' to a particular experiencer). The sense in which the nature of physical events may be said to be ' public ' is that physical events are describable in terms that are at any rate *covariant* with respect to the differences in (or transformations between) the spatio-temporal details of location and bodily constitution of different experiencers. In short the idea of a ' public ' world of physical events is a construction based on the covariance [1] found in the experiential events of differently situated and constituted observers, discovered by the inter-observer communication achieved through the medium of language. (' Language ' being understood in its most general aspect, including the language of gestures, imitative signs and the abstract symbols used in formal logic and mathematics.)

---

[1] For references see T. E. Allibone, ‘ Lightning and Spark Phenomena,’ *Nature*, London, 1949, **163**, 708-710

This does not mean that all *physical* events and objects are mere ' fictions ' ; or that their ' reality ' is only derivative from their participation in *experiential* events. To conclude this would be to make the sufficiently exposed ' Idealist ' fallacy. All that is implied is, first, that any *knowledge* of *physical* events, and thus of processes in *physical objects*, is inferential and never direct knowledge ; and, secondly, that the inferences by which such knowledge is reached have various degrees of probability, and always are based, in the last analysis, upon those sense-data that are the material for logical constructions, and which are furnished by experiential events.

But everyone normally believes that events, which are similar in structure to the ones we thus infer, when, as we should normally say, ' we perceive ' a physical event, go on all the time, quite irrespective of whether or not we happen to have any cognitive relationship to them. And we all in practice believe that such ' unobserved ' events can, and do, actually have a causal influence over other events, about which we do know through processes of inference and logical construction, based upon the data furnished by experiential events. And indeed this causal influence is the main significance which unobserved physical events have for us. There is no difficulty in my view about the conception of such events, provided we admit the possibility of ' unsensed sensible events,' which are similar in kind to the ones that we do happen to sense in course of our experience, i.e. provided we admit with Russell the possibility of ' unsensed sensibilia.'

---

[1] Not ' *invariance* ' as some writers, e.g. Max Born, have suggested. Vide *The Natural Philosophy of Cause and Chance*, Oxford, 1949

I hope I have now said enough to indicate as clearly as is possible, within the limitations of a paper of this kind, what I am taking about when I say that, in my analysis of the phenomenon of the specious present, I am concerned primarily with *experiential* events. Whereas, when I come to discuss the significance of ' the uncertainty principle in quantum theory,' I am mainly concerned with *physical* events.

I must now go on to the specific descriptions of usage needed to express key concepts of my theory. I shall consider these as briefly as possible.

(*a*) *The distinction between the transitory and extensional aspects of time.* There are two aspects to every process in time, which are distinguishable by reflective analysis, though they are invariably found together in concrete fact.

I *The transitory aspect* is the one involved in the general fact that any events which we can think of as happening at a certain particular time, must begin by being ' future ' (before that certain time), then become ' present ' (at that certain time), and finally become ' past ' (after that certain time). Thus, as I write now in the morning, my dinner engagement fixed for 8 p.m. tonight is still future, though it will (I hope) later become present, and finally past.

This aspect has also been called the ' becoming ' of events in time. It is clearly different from the second aspect which I have distinguished as :

II *The extensive or extensional aspect of time.* When in the first section of this paper I claimed that the paradox, inherent in current accounts of the phenomenon of the specious present, could only be resolved by the postulation of two time dimensions, I referred, of course, to this extensive aspect of time. This is the aspect representable graphically by means of stretches of points on lines, the positions of such points of time having an ordinal relationship describable in terms of the elementary relation ' earlier (or later) than.' This elementary relation, from the standpoint of pure logic, can be regarded as similar to other relations generating order among the elements of point-sets. Our assumption of a second time-dimension implies that there exists a second way of ordering the constituents of a temporal process, by means of a relation similar in structure to the relation of ' before (or after) ' or ' earlier (or later) than.' The two time-orders thus generated would have the same complete *logical* independence, or mutual orthogonality, as the three extensive aspects of *space-relations*, commonly described by three orthogonal cartesian coordinate axes. This does not entail that the 'becoming' aspect of time is *two*-dimensional.

(*b*) *The phases of events.* All events of which we have experience, manifest some finitely extended duration, and we never experience a *strictly instantaneous event.* All experiential events comprise thus a set of 'event-phases,' some of which precede others. By the term 'event-phase' I mean any distinguishable component which is a (phase-temporal) part of an event (i.e. a component whose duration is less extensive in phase-time than that of the event as a whole); remembering that while there must be a certain arbitrariness in deciding what counts as 'one event,' yet there can be no doubt that an occurrence which is punctiform in its extension in transition-time must be regarded as being in some sense 'one event.' But of course I do not deny that we often use the word 'event' of an occurrence with a long duration in both phase and transition time.[1]

Event-phases can have various extensions and ordinal relations in the dimension of phase-time. Some event-phases begin or end before others, yet what may be called the total event, comprising them, falls within the span of one specious present or total 'momentary experience.' For instance, when I see a flash of 'forked' lightning (an occurrence which often falls within one specious present), I can see that one prong of the fork-like flash begins before the other prong is seen. Each seen prong of the total directly experienced lightning event would be an instance of what I mean by the phrase 'event-phase of the total event.' (The 'total event' in this example would consist of the seeing of the whole flash of lightning.)

(*c*) *Coincidence of event-phases.* Two or more event-phases may be said to 'coincide' when their temporal extensions have a relation which is similar in structure to the one holding between spatial extensions of figures or volumes, when these latter have been conceived to be 'superposed,' in the classical so-called 'proofs of congruence' used in Euclidean geometry. There two figures or volumes are said to be 'congruent' when they can be carried into exact coincidence with each other by one of the operations comprised in the so-called 'group of Euclidean motions.' Thus the notion of 'coincidence' of event-phases is to be conceived on the analogy of Euclidean superposition of spatial extensions, rather than the non-classical concept of superposition, which occurs in quantum theory. The latter, which we describe in the third section of this paper, corresponds, on the physical side, to the psychological concept of ' compresence,' to which I now come.

---

[1] For example, the usage of the phrase 'historic event,' e.g. a speech in Parliament: this would occupy a considerable stretch of both transition and phase-time points.

(*d*) *Compresence.* The *directly experienceable* psychological relation, of compresence, is the one which holds between all the elements in a given particular experiential event, that fall within the specious present of the same experiencer. We have already given an instance of this kind of experiential situation in Section 1 of this paper; namely, the experience described by Stout, of hearing a postman's ' rat-tat ' upon a door knocker, the event-components of which are manifestly *successive*, though it is for the experiencing subject ' as if the whole (event) were present together.' In such situations the event-components comprise a set of event-phases which are manifestly successive in respect of some time-order, though they fall within the boundaries of a single event that is presented as a whole to the experiencer ; in the sense that the total experiential event comprising them occurs, for the experiencer, ' as if the whole were present together.' And Russell (to whom I am indebted for this use of the term ' compresence ') brings out another essential feature of the relationship, when he says, for instance :

> If I see something and at the same time hear something else, my visual and auditory experiences have a relation which I call ' compresence.' If at the same moment I am remembering something that happened yesterday and anticipating with dread a forthcoming visit to the dentist, my remembering and anticipating are also ' compresent ' with my seeing and hearing. We can go on to form the whole group of my present experiences and of everything compresent with them.[1]

Later on he says : ' In virtue of being compresent they constitute a single structure. One such structure may be called a " total momentary experience." '[2]

(*e*) *Temporal process.* All temporal processes involve a set of event-phases which have ordinal relationships to each other of the kind I have already mentioned (above, *b*), viz, the relation of ' earlier (or later) than,' which I shall call ' precedence ' for short. Besides this extensional relationship, events in a process have also a ' becoming' or transitory aspect in that, generally speaking, only a sub-class, at most, of the set is regarded as being actually present at any given moment. In the process of eating my dinner, for instance, the soup precedes the fish and the fish the meat (if I am sufficiently opulent).

---

[1] *Human Knowledge*, p. 312

[2] Ibid. p. 312

But at any moment only a sub-class of the eating-event-phases is compresent (unless I am a pig).

But because two distinguishable event-phases, one of which precedes the other in phase-time, can yet be compresent in one speciously present moment of transition-time, we must clearly realise that a phase of an event can *precede* another phase in one dimension of time, while yet the two are *compresent* together in another time-order. Then we shall have to say in such cases that, while the first event-phase 'precedes' the second in respect of one dimension of time, yet in a sense it does not '*wholly* precede it' (i.e. in *both* dimensions).

(*f*) *Gravitas or relative weight in temporal superposition.* It is clear, however, that different event-phases may contribute as components to the resultant 'single structure' of a 'complete complex of compresence' which falls within a specious present, with various weights. For instance, it is a well-known psychological fact that a given sensory component (such as a colour sense-datum like a visible flash occurring in someone's visual sense-field) displays a particular kind of variation in the way in which it is presented in a specious present. Alexander [1] has emphasised this kind of variation in the following passage, apparently based upon experimental data obtained by Bichowsky and reported in an article entitled, 'The Mechanism of Consciousness: pre-sensation.' [2] Alexander says, '. . . for in vision anyhow we have, corresponding with "after-sensations," "before-sensations" during which a color sensation is gradually rising to its full intensity or saturation.' Neither of the terms here used by Alexander, 'intensity' or 'saturation,' is satisfactory for the description of the feature I am talking about. For both of these terms are already used by psychologists and sensation-physicists in senses which are not related (directly, at any rate) to the temporal characteristic I am discussing. For it is certainly possible for two visible flashes of light, or patches of colour, to have the same intensity or saturation in the usual sense (that is, to be both equally sensibly bright or free from admixture of grey); while yet, if the one preceded the other in phase-time within a single specious present, then they could not have in my sense the same 'intensity' or 'amplitude of presentation' to the experiencer. We need a new term here to convey the idea of 'relative weight in the total superposition structure of a specious present.' And to express this notion I choose the word 'gravitas,' as this seems sometimes to be used in the English language to mean 'weight of presence' (usually applied in ordinary speech to the 'weight of presence' manifested by a human personality).

---

[1] S. Alexander, *Space, Time and Deity*, London, 1920, I, 121

[2] *American Journal of Psychology*, 1925, **36**, 588 ff.

(*g*) *Instants and Moments.* Finally we come to the description of the ultimate elements of the two distinct dimensions of the extensive aspect of time, which we have provisionally distinguished by the terms 'transition-time' and 'phase-time' respectively. The ultimate element or 'point' of phase-time is the 'instant'; which I define on the basis of Russell's definition (*Human Knowledge*, p. 289), when this definition has been modified so as to allow for a second time dimension :

I *Instants or points of phase-time.* An instant or point of phase-time is a class of event-*phases*, having the following two characteristics :

(i) All the event-*phases* in the class coincide ; (ii) no event-*phase* outside the class coincides with every other member of this class of event-*phases*.

If a set of event-phases has these two characteristics it will follow that no event-phase of the class *precedes* any other event-phase which is a member of the class. It will also follow that no event-phase which is outside (i.e. not a member of) the class can coincide with every member of the class. These relationships could only be exemplified by event-phases which had no finite extension (i.e. duration) in phase-time : for if an event-phase has such a finite extension, lasting say for a stretch of time $\triangle t$ seconds, there would necessarily be a sub-phase of it lasting $\frac{\triangle t}{2}$ seconds, whose later boundary $t + \frac{\triangle t}{2}$ did not coincide with *every* member of the class ; for instance, it would not coincide with a member occurring at phase-time t. The later boundary of this sub-phase of the event would then *precede* (and would *not coincide with*) phases of the sub-phase $t + \frac{\triangle t}{2}$ to $t + \triangle t$. But although an 'instant' thus defined is certainly without *extent in phase-time*, yet an instantaneous event-phase will in general (e.g. if it is not the last event-phase of my life) be a component in, or be comprised within, a series of specious presents which are successive in *transition-time*. We say more of this later.

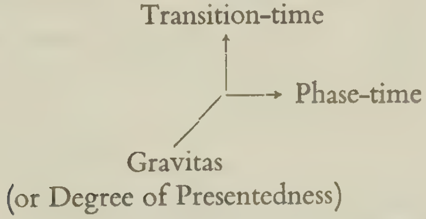
II *Moments or points of transition-time.* All that is now necessary in order to give a description of my usage of the word 'moment,' meaning a point of 'transition-time' (as distinct from a point of phase-time), is to take the description of the usage of ' instant ' given above, and to substitute the word ' event ' for ' event-phase,' and ' compresent ' for ' coincide.' A ' moment ' will then be a class of *events* such that : (i) all the *events* in the class are compresent ; (ii) no *event* outside the class is compresent with every other member of this class of *events.* It will then follow that, if a set of events fulfils these two conditions, no member of the set *wholly* precedes, or is *wholly* preceded, in transition-time, by any other event which is a member of the set. An event belonging to a class of this kind may indeed properly be called a ' momentary event,' in recognition of its occupying a single point of transition-time. Though it is *not* instantaneous or punctiform in phase-time, but involves a spread of compresence which is finitely extended in that dimension. However an ' instant ' in phase-time may contain components which, though they are all coincident in that dimension are yet presented in a succession of specious presents that occupy a finitely extended spread of transition-time ; so in this sense an ' instant ' may contain event-phases which are components in a succession of moments in transition-time, though in respect of the phase-time order these phases are all strictly coincident or contemporary.

Thus logically there is complete symmetry between the two time dimensions we postulate : in the sense that as far as experiential events go, we find that, just as there is a class of instants finitely extended in phase-time, comprised within a single point or ' moment ' of transition-time, so there is a class of moments through which an instantaneous event-phase is presented to a particular experiencer. (This symmetry, in respect of the two dimensions of time, is brought out clearly in a note on the mathematical logic of my theory which Professor C. D. Broad has been good enough to supply me with and which appears at the end of this section of this paper—see especially *Assumption 5*).

This usage will apply also to physical events once a physical usage of ' compresence ' has been described. A physical interpretation of these notions is described in my next section. We shall see there that, just as on relativity theory each piece of matter carries its own peculiar local coordinate system for expressing eigen-times or proper durations in the extensive aspect of temporality, so each interaction between a material system and a field of force, which eventuates in a *transition* from one energy-level to another, has its own peculiar range of compresence defined by the uncertainty principle of quantum theory. Thus there will be local modes of time-keeping in a quantum theory sense as well as in a relativistic sense : the quantum sense being that in which the minimum range of phases which are compresent for a particular transition in phase-time is given by Heisenberg's energy-time uncertainty relationship $\triangle t \,.\, \triangle E \sim h$.

We can now come at length to the completion of the solution of the paradox of the specious present. It is to make the distinction we have already drawn between phase-time and transition-time. An experiential event cannot begin until its earlier phases have been comprised in an event-complex which has begun in transition-time. But owing to the logical independence of these two dimensions in the extensive aspect of temporality, and to the indeterminacy in the actual linkage found empirically to hold in fact between these time-orders, there is no reason why an event, whose phases occupy a spread of $\triangle t$ instants in phase-time, should not occur *as a whole* in a single moment of transition-time ; where the extent of $\triangle t$ depends upon the contingent psychological or physical conditions and circumstances of the occurrence. (Indeed this is the sort of occurrence which quantum theory envisages, whenever one steady state of an atomic system changes by a discontinuous quantum-transition into another steady state.)

According to this two-fold view of the extensive aspects of temporality, then, a three-dimensional figure such as this will be essential to represent the structure of a specious present.



It is easy to see how this enables us to avoid the paradox attending the one-dimensional theory of temporality. For the successiveness manifested in a specious present is between event-phases, which are, in respect of transition-time, contemporary or compresent. The spread or extent in phase-time comprised within a single event which is punctiform in transition-time depends on the conditions surrounding the transition-event. We shall be concerned shortly with a particular kind of transition-event, namely, the reception of light quanta by human eyes, i.e. with certain sub-atomic phenomena. In the realm of sub-atomic physics the spread of compresence is very small. But, as we shall see, there is reason to believe this spread increases when a highly resonant-coherent structure, such as a human brain, is concerned. And the inner psychological aspect of a brain-event such as the experience we call having a sensation (or in other language, perceiving a sense-datum), will have a correspondingly enlarged spread of compresence.

The succession in time, which is experienced in a specious present, is provided for by the relations of precedence holding in phase-time between earlier and later phases of ' single structure,' which is the complete complex of compresence, and constitutes the total momentary experience. The unitariness of the event which makes it necessary to say, with Stout, that ' it is as if the whole were presented together,' is provided for by the fact that the whole complex of compresence occupies a single moment, i.e. a point, in transition-time.

We shall have to picture the specious present in terms of a very thin but yet finitely thick section or ' saddleback,' if we adopt William James' expression, slipping along a track in the plane of the phase-time and transition-time axes, by a process of parallel displacement. At each point of the transition-time axis, representing a moment of transition-time, a finite range of phase-instants are compresent ; with a finite degree of gravitas, or weight of presence, in the specious present.

The physical counterpart to this would be the occurrence, at different moments of transition-time, of events characterised by a certain amplitude of energy, and spread over a certain range of instants : that is to say the occurrence of events having certain quanta of action. And, on the kind of double aspect theory of psycho-neural parallelism which I am advocating, these events would be the outer physical aspects of occurrences whose inner psychological aspects are experiences. The objective happenings are on my view ' neutral,' having both outer physical aspects and inner psychological aspects, with a one-one correspondence between them.

The upshot on the psychological side, is that, on my view, we must treat the events which are experienced in a single specious present as being a complete complex of compresence in Russell's sense, when this is interpreted as having a two-fold dimensionality in its extensive temporal aspect. Such a complex of compresence, or total momentary experience, can be regarded structurally as the resultant of the superimposition of a range of event-phases. These event-phases are all compresent together ; but they may be conceptually divided up into instantaneous phases, each of which occupies a point or ' instant ' of phase-time. Different event-phases will make contributions to the resultant complete complex of compresence with varying weights ; and this variable factor of weight in the superposition process is what I call ' gravitas,' meaning by this term a *non-temporal* factor representing the analogue of amplitude, or weight, in the superposition of wave functions in physics. We can then say that the contribution which each event-phase makes to a total momentary experience is proportional to its gravitas, or ' degree of presentedness.' And if the standpoint of psycho-physical parallelism, common to Russell's theory and mine, is adopted, we can extend this analysis to physical events and processes as well. Gravitas (or degree of presentedness) will then be the psychological analogue of the weight or amplitude of wave-functions in a quantum-superposition of states in a dynamical system which undergoes changes in transition-time. It will be as if at each unit of transition-time, corresponding to a total momentary experience, there is a full fledged spread of compresent event-phases, between which relations of ' earlier than ' and ' later than ' obtain in phase-time. That some of these event-phases precede others in phase-time is important ; but the essential point is that nonetheless they are all contemporary or compresent, at a particular moment, though some will fade out before others.

H. A. C. Dobbs

A Logistic Analysis of the Two-fold Time Theory of the Specious Present, by Professor C. D. Broad (expressed in the symbolism of *Principia Mathematica*).

General Notions. Let us say that an *instantaneous event phase occupies* an *instant t* ; and denote this by

$$ePt.$$

Let us say that it is presented to a subject s at a *moment* T and with a certain gravitas or *degree of presentedness g* ; and denote this by

$$e\prod_s^g \mathrm{T}.$$

It will be convenient to write for $(\exists g)\, e\prod_s^g \mathrm{T}$ the simple formula

$$e\prod_s \mathrm{T}.$$

N.B.—If it is held that degree of presentedness is a property only of *processes* with a finite duration in the *t*-dimension, what I have called ' degree of presentedness ' will be the rate of change of what will then be called ' degree of presentedness ' with *t*. Cf. ' total utility ' and ' marginal utility ' in economics.

Assumptions 1.1 *and* 1.2. I assume that instants form a continuous one-dimensional open endless series ordered by a relation I, and that *moments* form a series of the same kind ordered by a relation M. I shall write

$$\mathrm{T}' >_{\mathrm{M}} \mathrm{T}$$

for ‘ the moment T′ comes later in the series of moments than the moment T.’ And

$$t' >_{\mathrm{I}} t$$

will have the same meaning with ‘ instant ’ substituted for ‘ moment ’.

> *N.B.*—I suppose that to call the series of instants and the series of moments the ‘ dimensions ’ of a single variable called ‘ time ’ entails that ‘ M ’ and ‘ I ’ are just two different names for the same relation. I think it is best not to assume this tacitly, but to assert it explicitly if one wants to hold that view.

Assumptions 2.1, 2.2 *and* 2.3. I assume that

$$ePt \text{ and } ePt' : \rightarrow t = t', \text{ i.e. } \mathrm{P} \,\epsilon\, Cls \rightarrow 1 \quad . \quad . \quad (2.1)$$

Also $$e \prod\nolimits_s^g \mathrm{T} \text{ and } e \prod\nolimits_s^g \mathrm{T}' : \rightarrow \mathrm{T} = \mathrm{T}', \text{ i.e. } \prod\nolimits_s^g \epsilon\, Cls \rightarrow 1 \quad . \quad (2.2)$$

But $\prod_s$ is *not* $Cls \rightarrow 1$, for the same instantaneous event may be presented to S (though not with the same degree of presentedness) at several different moments. . . . . (2.3)

Since an event e can occupy one and only one instant we can talk of ‘ *the* instant occupied by e.’ This will be denoted in the usual way by

$$\overset{\cup}{\mathrm{P}}\text{‘}e.$$

The class of moments at which an event e is presented to s will be denoted, as usual, by

$$\overleftarrow{\prod}_s\text{‘}e.$$

Assumption 3. I assume that, if e and e' occupy the same instant, then the class of moments at which e is presented to s is identical with the class of moments at which e' is presented to s, i.e.

$$\overset{\cup}{\mathrm{P}}\text{‘}e = \overset{\cup}{\mathrm{P}}\text{‘}e' \rightarrow \overleftarrow{\prod}_s\text{‘}e = \overleftarrow{\prod}_s\text{‘}e'. \quad . \quad . \quad . \quad (3)$$

Now we can define the class $\lambda_{s,t,e}$ as the class of all the moments at which an event e which occupies the instant t is presented to s. Thus

$$\lambda_{s,t,e} = (\hat{\mathrm{T}})[ePt \,.\supset_e .\, e \prod\nolimits_s \mathrm{T}].$$

But in virtue of Assumption (3) $\lambda_{s,t,e} = \lambda_{s,t,e'}$. So we can drop the e and denote the class simply by $\lambda_{s,t}$. So $\lambda_{s,t}$ is the class of *moments* such that *any* instantaneous event occupying the *instant* t would be presented to s at *all* these *moments.*

Now there is obviously another class complementary to $\lambda_{s,t}$. This is the class of *instants* such that an event which occupies *any* of these *instants* is presented to s at a given *moment* T. We can denote this by $\mu_{s,\mathrm{T}}$. We have

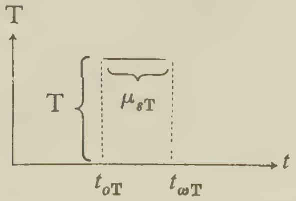
$$\mu_{s,\mathrm{T}} = \hat{(t)}[ePt \,.\, \supset_s . \, e \prod\nolimits_s \mathrm{T}].$$

Empirical facts about normal Specious Presents:

(1) For any moment T the members of $\mu_{s,\mathrm{T}}$ form a *single continuous* segment of *finite* length ordered by the relation I. Thus we can talk of the upper and the lower bounds of $\mu_{s\mathrm{T}}$, and we have

$$(\mathrm{T}) : \mathrm{E} \,!\, \mathrm{limin}_{\mathrm{I}} \text{‘} \mu_{s\mathrm{T}} \;\&\; \mathrm{E} \,!\, \mathrm{limax}_{\mathrm{I}} \text{‘} \mu_{s\mathrm{T}}.$$

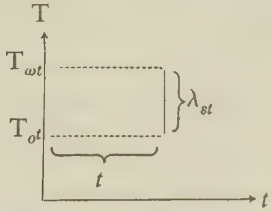
It will be convenient to denote these instants by t_{ot} and $t_{\omega t}$ respectively.



(2) For any instant t the members of λ_{st} form a *single continuous* segment of *finite* length ordered by the relation M. Thus we can talk of the upper and the lower bounds of λ_{st}, and we have

$$(t) : \mathrm{E} \,!\, \mathrm{limin}_{\mathrm{M}} \text{‘} \lambda_{st} \;\&\; \mathrm{E} \,!\, \mathrm{limax}_{\mathrm{M}} \text{‘} \lambda_{st}.$$

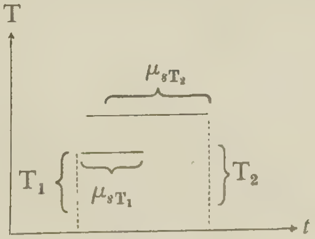
It will be convenient to denote these moments by T_{ot} and $\mathrm{T}_{\omega t}$ respectively.



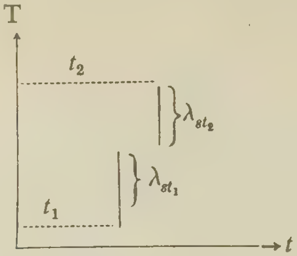
(1.1) If $\mathrm{T}_2 >_{\mathrm{M}} \mathrm{T}_1$ then $\mathrm{limin}_{\mathrm{I}} \text{‘} \mu_{s\mathrm{T}_2} >_{\mathrm{I}} \mathrm{limin}_{\mathrm{I}}\, \mu_{s\mathrm{T}_1}$,

and $\mathrm{limax}_{\mathrm{I}} \text{‘} \mu_{s\mathrm{T}_2} >_{\mathrm{I}} \mathrm{limax}_{\mathrm{I}}\, \mu_{s\mathrm{T}_1}$,

i.e. $t_{o\mathrm{T}_2} >_{\mathrm{I}} t_{o\mathrm{T}_1} \;\&\; t_{\omega\mathrm{T}_2} >_{\mathrm{I}} t_{\omega\mathrm{T}_1}$.



(2.1) Similarly, *mutatis mutandis*, for λ_{st_1} and λ_{st_2} if $t_2 >_I t_1$,

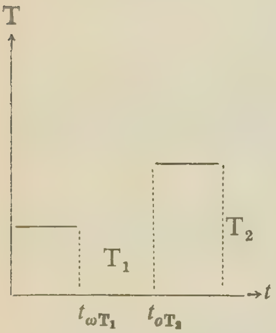


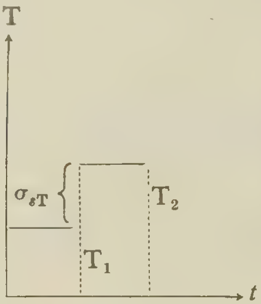
(1.2) There is a certain magnitude σ_{sT}, such that,

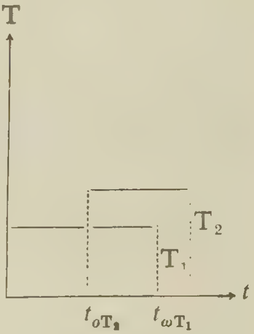
if $T_2 - T_1 > \sigma_{sT}$, then $t_{\omega T_1} <_I t_{oT_2}$;

if $T_2 - T_1 = \sigma_{sT}$, then $t_{\omega T_1} = t_{oT_2}$;

and if $T_2 - T_1 < \sigma_{sT}$, then $t_{\omega T_1} \in \mu_{sT_1}$ and $t_{oT_2} \in \mu_{sT_1}$.





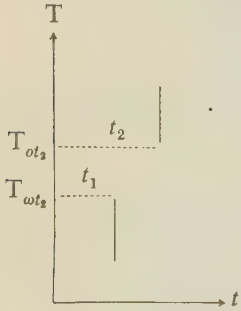


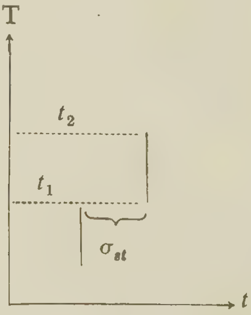
(2.2) There is a certain magnitude σ_{st}, such that,

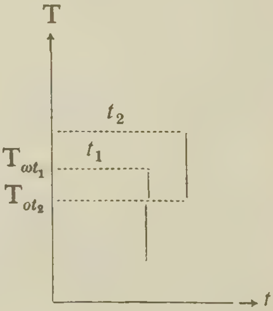
if $t_2 - t_1 > \sigma_{st}$, then $T_{\omega t_1} <_M T_{ot_2}$;

if $t_2 - t_1 = \sigma_{st}$, then $T_{\omega t_1} = T_{ot_2}$;

and if $t_2 - t_1 < \sigma_{st}$, then $T_{\omega t_1} \in \lambda_{st_2}$ and $T_{oT_2} \in \lambda_{st_1}$.







(1.3) If T_1 and T_2 are different, then μ_{sT_2} and μ_{sT_2} are not identical, and conversely i.e.:

$$T_1 \neq T_2 \rightleftarrows \mu_{sT_2} \neq \mu_{sT_2}$$

(2.3) If t_1 and t_2 are different, then λ_{st_1} and λ_{st_2} are not identical, and conversely: i.e.

$$t_1 \rightarrow t_2 \rightleftarrows \lambda_{st_1} \neq \lambda_{st_2}.$$

(1.4) As T_2 approaches T_1 so t_{oT_2} approaches t_{oT_1}; i.e.

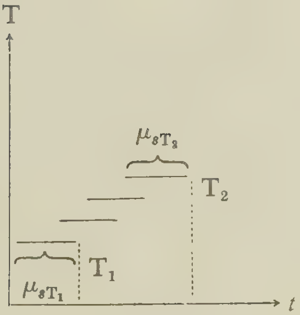
$$\underset{(T_2-T_1)\to 0}{Lt} \cdot \left(t_{oT_2} - t_{oT_1}\right) = 0.$$

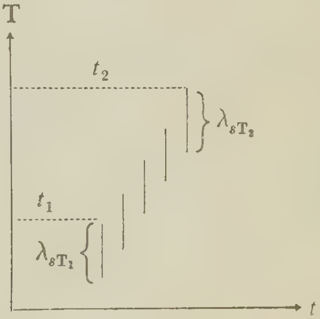
(1.41) Similarly $\underset{(T_2-T_1)\to 0}{Lt} \left(t_{\omega T_2} - t_{\omega T_1}\right) = 0.$

(2.4) As t_2 approaches t_1 so T_{ot_2} approaches T_{ot_1}; i.e.

$$\underset{(t_2-t_1)\to 0}{Lt} \left(T_{ot_2} - T_{ot_1}\right) = 0.$$

(2.41) Similarly for $T_{\omega t_2} - T_{\omega t_1}$.





THE NATURE OF SOME OF OUR PHYSICAL CONCEPTS

III*

In this third lecture we shall examine some of the concepts by which we describe various elementary electrical phenomena in massive conductors carrying electrical currents. As in the second lecture, we shall be largely concerned with separating the instrumental from the paper-and-pencil operations, in seeing how we construct our paper-and-pencil operations so as to satisfy certain basic verbal demands, and in examining what sort of correspondence is attainable between the verbal demands and the instrumental operations.

It is, I think, the general opinion that the classical scheme of description of electrical phenomena in conductors, as contained for example in Maxwell's equations, is adequate for a description of all the macroscopic aspects of current flow. I believe, however, that this is not the case, but that when we push the implications of our paper-and-pencil operations to the limit, and demand that we be able to describe the result of any conceivable mental experiment, we shall find that we have to supplement the classical scheme in certain respects and modify it by recognising distinctions which can be ignored in the simpler situations. In order not unnecessarily to complicate the issues we shall, in most of the following, concern ourselves only with steady current flow in systems with no moving parts and of such small dimensions that the velocity of light propagation may be neglected. This means that the electric vector at every point can be calculated from the distribution of macroscopic static charges by the inverse square law and is therefore derivable from a potential function. The magnetic field can be calculated from the currents. We shall, however, allow the electrical system to be composed of different metals with temperature differences between the parts (which may be maintained at steady values by appropriate thermal reservoirs) so that the system is the seat of thermo-electromotive forces and of the conversion of thermal into electrical energy. We shall find, I think, that the conventional macroscopic scheme of description is not adequate to represent all the interactions between electrical and thermal energy, but it will have to be supplemented.

---

* The last of three lectures delivered in the University of London (University College), on 24th, 26th and 28th April, 1950.

Our equations, that is, our paper-and-pencil operations, employ a current vector which defines a density of current flow at every point of the interior of a massive conductor. This current density is, in the first instance, a constructed quantity as is any other thing pertaining to the inaccessible interior of a solid. However, its instrumental implications are so varied and numerous, that we can pretty well give current density full instrumental status. For the total integrated current may be given independent status by several different methods, as by measuring the quantity of electricity deposited when the conductor is connected to a condenser of large capacity, or by measuring the chemical decomposition in a Faraday experiment in electrolysis, or by computing the magnetic field in surrounding space (and checking with experiment). Furthermore, the assumed detailed distribution within the conductor may be checked by dividing the conductor into filaments along the lines of current flow, replacing each filament by a physically independent conductor, and then checking the flow in each filament by one of the means just suggested.

The presence of a current at any point of a conductor involves, according to the conventional picture, a force acting on the current at that point to maintain it against the electrical resistance. This force has full paper-and-pencil significance through the equation

$$\text{current density} = \frac{\text{intensity of force divided}}{\text{by specific resistance}} \quad . \quad . \quad (1)$$

The specific resistance, of course, has full instrumental significance through independent measurement. The situation with respect to the instrumental significance of the force is somewhat more complex. In certain simple situations, as in the interior of a homogeneous massive conductor all at the same temperature, we could give a somewhat impractical instrumental significance in terms of the force that would be experienced by a microscopic test charge in a cavity excavated in the conductor, proper correction being made for the charges which would appear by induction on the walls of the cavity. Also, in this simple case we can give independent paper-and-pencil significance to the force in terms of calculations by the inverse square law from the total distribution of electric charge. But in the general case, in particular when there are chemical inhomogeneities or temperature gradients, the force cannot be calculated in this way, and we have to recognise the presence of a component in the force which is not given by the static charges. We may call this the ' non-field ' force, since it cannot be deduced by the field equations from the charge distribution. In general, we have to recognise the existence of a ' non-field ' force acting not only on currents but also on static electric charges. In particular, such a force, implied in the equations but almost never written out explicitly, is made necessary by the ' boiler pressure ' tending to blow the static charge off the surface of a curved conductor. The charge is held in place by a counter force, not deducible from the equations. This counter force is sometimes described as a ' non-electric ' force, but since in a sense any force acting on electricity is an ' electric ' force, it would seem that the term ' non-field ' force is perhaps a better description. Another example is afforded by any battery delivering a steady current. Within the battery current flows contrary to the direction of the field force derived from the static charges by the inverse square. There must be therefore within the battery a ' non-field ' force, and furthermore this ' non-field ' force is greater than the ' field ' force. It is obviously connected with the ' electro-motive-force ' of the battery, which in turn is connected with the energy delivered by the battery, so that the ' non-field ' force has two aspects. In the case of the simple battery, there is a simple relation between the two aspects of the ' non-field ' force, that is, the aspect relating to current flow against resistance and the aspect relating to energy. In the general case, however, the same simple relation does not hold between these two aspects as holds for a battery.

In addition to the two examples just given, there is in simple isothermal systems another ' non-field ' force. This is located in the surface of the metal, as is also the force equilibrating the ' boiler pressure ' of the surface charge, but it is not related to the boiler pressure and is connected with the Volta effect. It is well known that the surfaces of two different metals in contact with each other and all at the same temperature will come to a difference of electrical potential, characteristic of the two metals. Because of this difference of potential the space surrounding the metals becomes the seat of an electric force, which has full instrumental status since it may be measured with an exploring charge. Since the entire surface of either single metal is at the same potential, the metal being a conductor, it follows that the Volta potential difference demands a discontinuous jump in the potential where the two metals join. Mathematically a jump in potential demands infinite electrical charges spaced an infinitesimal distance apart. The facts of the Volta phenomenon can be explained by supposing the surfaces of the two metals to be the seat of a potential jump, and therefore of a double layer. This potential jump and consequent double layer may be situated on any of three possible surfaces, namely the two surfaces separating the metals from surrounding space and the surface on which the two metals are in contact. There is only one condition on these three double layers and the corresponding potential jumps, namely that together they give the observed instrumental Volta difference between the two metals. Evidently the one condition imposed by the Volta difference is not sufficient to determine the values in detail of the three jumps (but jump there must be somewhere).

The double layer on the surface of a conductor thus demanded by the Volta phenomenon does not have full instrumental significance. For a uniform double layer may be added to the surface of any isolated conductor without changing any instrumental result. For such a double layer has no effect at outside points, and within has the effect only of uniformly raising the potential everywhere by the same amount. Since this uniform increase of potential is accompanied by no change of electrical force, and since it is only electrical forces and not potentials that electrical instruments respond to, there is no way of giving instrumental meaning to the supposed double layer. This is a situation that makes our physical intuition profoundly uncomfortable (for the picture that we form of the double layer on the surface is drawn with the same physical elements, positive and negative charges, as have instrumental significance in other situations), and it does not seem right that we should not be able to determine uniquely what double charge is on any given surface. In the absence of any instrumental method of giving the answer we may try to find the answer in the paper-and-pencil domain by connecting with some theory. It proves, however, that as long as we stay in the domain of macroscopic phenomena governed by the classical field equations, no theory gives a unique answer to this question. Historically there have been two important different points of view. Lord Kelvin believed that the entire Volta jump originated in the surface of separation of the two metals in contact, whereas Maxwell and Heaviside would have no potential jump at this surface (except for the small Peltier jump to be discussed later), and put the jumps at the surfaces separating the metals from surrounding space. Both protagonists attempted to prove their position by one sort of argument or another, but I believe that there are no macroscopic phenomena covered by the equations which permit a decision between these two. In particular, the contention of Heaviside that a decision could be reached on the basis of the transformations of energy I believe to be unsound, since the Poynting vector, in the regions where it is incapable of instrumental verification, that is, within the hypothetical double layers, is capable of so adjusting itself as adequately to account for any particular energy manifestation that may be demanded by any particular hypothesis about the double layers. More than this, in our present wider experimental range, in which are included such new operations as taking electricity through a surface into the surrounding space, as in the phenomena of electron emission, I believe that we are still unable to give full instrumental status to the existence of these potential jumps and double layers, and their meaning is still to be sought partially in the paper-and-pencil domain. This may appear unsatisfactory, but it is not unlike other situations. The instrumentally meaningless isolated double layer may remind one of the instrumentally meaningless single valued energy that our mathematics would find congenial. Both of these concepts find their instrumental meaning only in complex situations with a plurality of elements.

Let us now consider systems in which we have different metals and temperature differences, but no chemical action. We may typify this by the conventional thermoelectric circuit shown in Figure 1, in which we have two homogeneous metals A and B leading from a region all at one temperature, T_1, to a region all at another, T_2, within the regions the metals being in contact. Such an arrangement is the seat of an electric current which may be used to deliver energy through

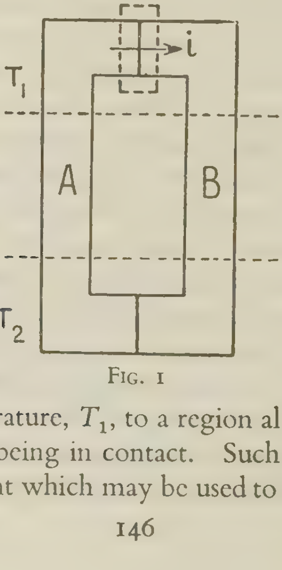


FIG. 1

a motor, and therefore is also the seat of an electromotive force. The energy delivered by the current has full instrumental status as does also the current, so that the E.M.F. also has full instrumental status through the definition,

Energy delivered by current in unit time equals
current $\cdot \times$ E.M.F. . . . . (2)

The 'energy delivered' here means mechanical work done by the motor plus the heating effect of the current in overcoming Joulean resistance. The latter has instrumental status, either directly, or through the equation; Joulean heating in unit time equals i^2R, R being measureable and with full instrumental status. By definition, the thermoelectric power of the circuit is the E.M.F. for unit temperature difference between the junctions. The thermoelectric power may be written as dE_{AB}/dT where E_{AB} is the total E.M.F. of a couple composed of the metals A and B, reaching from an arbitrary constant lower temperature to a variable upper temperature T. The order of the subscripts indicates that on closed circuit current flows from A to B at the hot junction. In general, dE_{AB}/dT is a function of the average temperature of the couple. The source of energy is thermal, there being no other possibility, such as chemical transformations. As a consequence certain thermal phenomena occur in the system when current is flowing which do not otherwise take place. There are two sorts of these phenomena. One takes place at the interface between the two metals in contact, and the other in the body of the homogeneous metal where there is a temperature gradient. At the interface between the two metals additional heat has to be supplied from some outside source when current flows to maintain the same temperature distribution in the circuit as before the current was made. This heat is called the Peltier heat. By experiment it is proportional to the current and fully reversible. It is denoted by P_{AB}, and is defined as the heat which has to be supplied from outside to maintain the original *status quo* of temperature when unit quantity of electricity is transported by the current through the interface in the direction from A to B. This heat absorption takes place within the solid metal at the interface across which current is flowing. Strictly it should perhaps be regarded as a paper-and-pencil quantity, but it would be a super-refinement not to accord it full instrumental status, because the total heat fed into the general neighbourhood of the junction from outside has full instrumental status, and the same value is found experimentally in a wide variety of dimensions and proportions for the circuits.

The second thermal effect takes place in the body of the metal when current flows through a temperature gradient. By definition it is the extra heat, in addition to the Joulean heat, supplied by the surroundings to maintain the original temperature *status quo* when unit quantity of electricity is transported by the current from one point to another one degree higher. This heat is called the Thomson heat, and for the metal A is denoted by σ_A, the subscript indicating that it is a function of the metal. It is also a function of the temperature. Within experimental error the Thomson heat, like the Peltier heat, is proportional to the current and reversible with it, and it has full instrumental status in the same sense as the Peltier heat.

There are relations between the two heats and the thermoelectric power of the couple. This topic has already been touched in the first lecture,[1] where it was mentioned that Kelvin first deduced these relations by an argument which he recognised as defective, by neglecting the irreversible aspects of the phenomena, which cannot be made to vanish (as they usually can in other similar situations) by any choice of the dimensions or manipulations of the rate of the process. It appeared that the same results could also be obtained, not by neglecting the irreversible phenomena, but by allowing them to occur in full swing and correcting for them by postulating that each of the two irreversible processes is accompanied by a characteristic increase of entropy. The formulas so obtained are,

$$P_{AB} = \tau \frac{dE_{AB}}{d\tau} . \qquad . \quad . \quad . \quad . \ (3)$$

$$\sigma_B - \sigma_A = \tau \frac{d^2E_{AB}}{d\tau^2} . \qquad . \quad . \quad . \quad . \ (4)$$

Every term in these formulas has full instrumental status, and the formulas have been abundantly checked by experiment. So long, therefore, as we are concerned only with the complete circuit we remain in the domain of completely instrumental quantities. But we leave this domain when we start talking about the action in parts of the circuit, as we are almost forced to when we start forming a theory of the action. For instance, in our theory we shall certainly want to apply the first law of thermodynamics, and this law, as we have seen, is applicable to any element of volume that can be cut out of the system. In particular we shall want to be able to say that the first law applies to the dotted element of volume in Figure 1. This means that in the steady state (which we are assuming) the net energy flowing out of the region is zero, since energy is not created, and to formulate this we find ourselves at once in the midst of constructional paper-and-pencil quantities. The construction that we adopt will depend to a certain extent on our theory.

---

[1] This *Journal*, 1951, 1, 257 ff.

Let us begin by examining the account that Heaviside gave of this situation. He said that since there is in the complete circuit an E.M.F. which is equal to the total heat absorbed by the whole couple, and since heat is absorbed at the interface, it is obvious that there is an E.M.F. in the interface equal to the heat absorbed there, that is, equal to the Peltier heat. But now this E.M.F. at the interface implies a ' non-field ' force which will tend to produce a current. Furthermore, since the E.M.F. is finite and the region in which it acts is of infinitesimal thickness, the corresponding ' non-field ' force must be infinite, and this by itself would produce an infinite current, the specific resistance within the metal being everywhere finite. The infinite ' non-field ' force must therefore be counterbalanced at the interface by an equal and opposite field force, and this means a double layer of static charge of strength equal to the surface E.M.F., that is, equal to the Peltier heat. This double layer means a difference of potential on opposite sides of the surface of separation of the metals equal to the Peltier heat. This means an equal jump in potential in the space immediately outside the free surfaces of the metals across the line of separation. In addition to this exterior jump accompanying the interior jump there may also be a jump due to the Volta jumps, which Heaviside located on the external surfaces of A and B. These Volta jumps play no part in the present picture, however, since the Volta effect is by itself not a source of energy. We neglect the Volta jumps for the present analysis therefore. How now can Heaviside meet the energy demands on the element of volume embracing the surface AB ? Heat energy is flowing in of amount iP_{AB}. What is the compensating outflowing energy ? It appears at once that there is such a flow of energy in the Poynting vector. Because in the narrow external girdle surrounding the surface *AB* there is an intense electric field because of the potential jump. There is also a magnetic field because a current is flowing. The magnetic field is circumferential and the electric field along the axis of the wire, that is, at right angles to the magnetic field. There is therefore a Poynting vector perpendicular to the external surface of the conductor, and on working out the mathematical values it is found that the total energy carried away on the Poynting vector from the neighbourhood of the junction is exactly equal to the heat input, or, per unit time, equal to iP_{AB} or $i\triangle V$, where $\triangle V$ is the potential jump across the interface. Everything therefore comes out as it should, and Heaviside felt justified in his point of view. But even so, the solution does not have full instrumental status, as indeed it cannot when it is reflected that the solution is not unique. For the double layer can be modified by any arbitrary addition, provided only that a compensating addition is made at the other junction. For although there is an additional energy flow under such conditions, it is a closed flow, out of one junction and into the other, and cannot be distinguished instrumentally from a flow into the Volta double layer on the external surface of the wires.

In additional comment on the solution of Heaviside it is to be kept in mind for the later argument that he postulated in the interface *AB* a local E.M.F. which was the same fraction of the total E.M.F. of the circuit as the local heat is of the total heat.

It is obvious that the state of affairs at the surface *AB* must be dealt with by some sort of construction. It would also be desirable if this construction should be unique, because then we might hope to be able eventually to give it full instrumental status. I think it is obvious, however, that as long as we have only the considerations of Heaviside we will never be able to find a unique solution, but there will always be freedom with respect to how the potential jumps are distributed among the three possible surfaces. Fortunately, it turns out that there is an entirely new sort of experimental consideration which enables us to find a unique solution in certain simple cases, and in the general case a solution unique except for an arbitrary constant.

The new order of experimental fact is afforded by the behaviour of single metal crystals. It is an experimental fact that two rods cut in different orientations from the same non-cubic metal crystal will function like two different metals in a thermocouple. That is, a thermocouple may be constructed from the parallel and perpendicular orientations of a single crystal as indicated in Figure 2. In such a couple Peltier heat is absorbed at the interface when current flows across the interface from one orientation to the other, and where there are temperature gradients there are Thomson heats, $\sigma_{\parallel}$ or $\sigma_{\perp}$ depending on whether the current flow is parallel or perpendicular to the crystal axis. These heats, as in the case of the ordinary couple, are point functions, characteristic only of the metal (and the direction of current flow), and not dependent at any point on what may be happening at other points of the circuit. Heaviside's point of view applied to this situation would demand that there is a jump of potential on going across a surface at which the orientation changes, and therefore a double layer at the interface characteristic only of the orientations. But this leads, for the crystal, to an impossible state of affairs. For consider the system at constant temperature represented by Figure 3, in which a piece has been cut out of a single crystal and reinserted in another orientation. Because it is at constant temperature, no currents can flow. Consider now the changes of potential encountered in describing the closed path *ABCDA*. By hypothesis in passing from *A* to *B* a jump of potential is encountered equal to $P_{\perp\parallel}$. From *B* to *C* no change of potential is encountered because this path is entirely within the single homogeneous metal in which the potential is constant, or otherwise current would flow. From *C* to *D* again exactly the same jump of potential is encountered as from *A* to *B* because the orientations are the same. Again from *D* to *A* there can be no change of potential. We have therefore returned to the starting point and the initial potential after encountering a total potential change of $2P_{\perp\parallel}$.

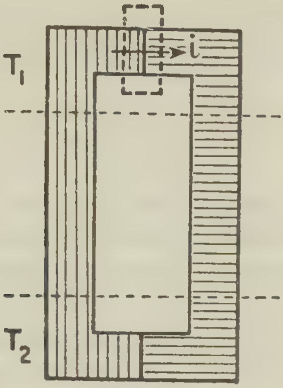


FIG. 2

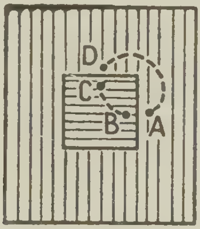


FIG. 3

Therefore $P_{\perp \parallel}$ must be zero and there can be no potential jump and no double layer on crossing a surface at which the orientation changes.

With this new knowledge, let us return to an examination of the first law as applied to the dotted region in Figure 2. As before, the Peltier heat $P_{\perp \parallel}$ flows into the region. But now there is no potential jump and no electric field in the girdle surrounding the wire and hence no Poynting vector to carry off the energy to compensate for the inflowing heat. What other possible source of energy flow is there in this situation? It is evident, by the principle of sufficient reason, that only the current can play a part here. We are apparently almost forced to 'say' that the current carries out of the region energy equal to the inflowing heat energy. Let us see where we get by postulating an energy convected by the current. In order to be satisfactory I think we would demand that this postulated convected energy have certain properties. It must be a 'point function,' that is, the energy carried by the current at a point in the conductor must be determinable in principle (i.e. by mental experiments) in terms of instrumental operations made at the point in question. In other words the convected energy at a point must not depend on what is going on in other parts of the system. If this were the case the detailed expression for the convected energy at a point would change with every change in the gross geometry of the system at distant points, and would indeed be a mere convention. These requirements in the present case mean that the energy convected per unit current may be a function of the current itself, of the material in which it is flowing, of the direction of flow with respect to the crystal axis, of the temperature, and of nothing else, because these are all there are at the point. Apply now the first law to the element of volume of Figure 2. Denote by U the energy convected in unit time per unit current. In unit time heat energy in amount $iP_{\perp \parallel}$ flows into the box, energy is convected in by the current in amount $iU_{\perp}$, where $U_{\perp}$ is written because on the entrance side current flows perpendicular to the crystal axis, and energy $iU_{\parallel}$ is convected out. This is all there is; in particular there is no Poynting flow because there is no double layer and no potential jump in the surface. Equating the energy flowing out to that flowing in gives,

$$i(U_{\parallel} - U_{\perp}) = iP_{\perp \parallel} \quad . \quad . \quad . \quad . \quad (5)$$

The current cancels. We may replace the Peltier heat by an expression in the Thomson heats by the two fundamental relations (3) and (4), obtaining,

$$U_{\parallel} - U_{\perp} = \tau \int_0^{\tau} \frac{\sigma_{\parallel} - \sigma_{\perp}}{\tau}\, d\tau \quad . \quad . \quad . \quad (6)$$

The lower limit of temperature integration is taken as 0° Absolute because of the third law of thermodynamics, which demands that Peltier and Thomson heats vanish at 0° Absolute. The solution of this equation for U is

$$U_{\alpha} = \tau \int_0^{\tau} \frac{\sigma_{\alpha}}{\tau}\, d\tau + f(\tau),$$

where the subscript α indicates that the expression is to be formed for either the parallel or perpendicular direction of flow. The temperature function of integration $f(\tau)$ is not a function of orientation. Since in an isothermal conductor steady flow of current is accompanied by no thermal effects (except Joulean heating), f must reduce to a constant, independent of temperature and dependent only on the substance. We have then finally,

$$U_{\alpha} = \tau \int_0^{\tau} \frac{\sigma_{\alpha}}{\tau}\, d\tau + \text{const (material)}. \quad . \quad . \quad (7)$$

Next consider an element of volume in the homogeneous conductor where current flows through a temperature gradient at constant orientation, as indicated in Figure 4. We suppose the conductor lagged laterally, so that there is no entrance of heat by lateral conduction, and the only heat entering the element is by conduction along the axis in virtue of a longitudinal temperature gradient. Now there are several energy manifestations in this element of volume, and each of them demands its own verbal formulation. These different formulations should be consistent with each other, and if they correspond to anything more than mere conventions, should be capable of some sort of instrumental contact. There are at least four of these energy aspects : there is the total energy in all forms entering and leaving the region, there is the energy associated with the current, there is the total energy associated with the matter within the region, and there is energy in the form of heat associated with the matter.

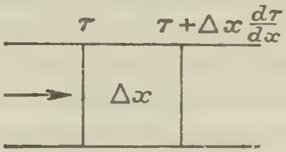


FIG. 4

Consider first the total energy. Since energy is not created we have to verbalise this as, ' total energy entering region is equal to total energy leaving region (for unit time), or *net* energy entering equals zero.' Energy gets into and out of the region in only three ways ; by convection by the current, by thermal conduction, and by flow on the Poynting vector through the lateral surface, where there is a Poynting vector normal to the surface. Writing in order the net energy of these three processes gives the equation

$$-\frac{d}{dx}(iU)+\frac{d}{dx}\left(\kappa\frac{d\tau}{dx}\right)-i\frac{dV}{dx}=0. \quad . \quad . \quad (8)$$

Here κ is the longitudinal thermal conductivity and the subscript has been omitted from U since the orientation remains constant for these considerations and it is not necessary to specify it.

Next consider the energy associated with the current. I think we are under compulsion to verbalise this as follows, ' Excess energy carried out of the region over that carried in by the current is equal to the total energy acquired by the current within the region.' We already know how to write the excess energy convected out. The energy acquired by the current within the region consists of three parts. There is the energy acquired as Thomson heat. In this particular set-up in which there is lateral lagging to prevent lateral entrance of heat, the source of this Thomson heat energy must be described as the ' matter ' within the element. In an alternative set-up, in which there is no lateral lagging, but excess heat is fed in laterally when current flows so as to maintain the temperature *status quo*, we would say that the current extracted its Thomson heat from the lateral flow from the surroundings. There is also another type of interchange of energy between current and matter within the element, namely in the form of Joulean heat. Matter acquires Joulean heat, and the current loses an equal amount of energy. Finally, the source of the Poynting outflow from the region must be described as the current. The mechanism of this flow is by the current flowing through the potential gradient of the field. Writing out the explicit expressions for these three energy acquisitions by the current gives for the equation of energy balance on the current,

$$\frac{d}{dx}(iU)=i\sigma\frac{d\tau}{dx}-i^2r-i\frac{dV}{dx} \quad . \quad . \quad . \quad (9)$$

Here r is resistance per unit length.

We have now formulated our first two demands, with respect to total energy and energy associated with the current. Our next two demands were with regard to energy associated with the matter. Since the matter is in a steady state, there can be no change in the associated energy within the region, so that our two formulations become ; ‘ net energy associated with matter leaving region is equal to energy acquired by matter inside,’ and ‘ net energy in the form of heat associated with matter leaving the region is equal to energy in form of heat acquired by matter inside.’ These two demands here become identical, for the only energy associated with matter with which we are concerned is heat energy, since there is no motion of the parts and there are no elastic stresses, and there are no electrical forces acting on matter, so that there is no mechanical energy involved in any form. The heat associated with matter entering or leaving is conduction heat. Heat is acquired by the matter inside in two forms ; as Joulean heat from the current and as Thomson heat from the current (this is negative because this heat is imparted). The equation of energy balance on the matter is therefore,

$$-\kappa\frac{d^2\tau}{dx^2}=i^2r-i\sigma\frac{d\tau}{dx}\quad . \quad . \quad . \quad . \ (10)$$

All the terms in this last equation have full instrumental significance, so that it is capable of direct experimental check. This check is met within experimental error.

In fact, in practice the equation is usually inverted and used as a means of determining σ, since temperature gradients are easier to measure than the small amounts of heat involved in a direct determination of the Thomson heat.

We must demand that our method of verbalising the different aspects be consistent. This is at once seen to be the case, because on eliminating U between equations (8) and (9) we obtain (10).

What account do we thus obtain of the detailed action in different parts of the circuit ? It is obvious that the thermoelectric circuit involves ‘ non-field ’ forces, because otherwise current would not flow. How are these ‘ non-field ’ forces distributed ? The ‘ non-field ’ force is defined through Ohm’s law by the equation

$$i=\text{total force}/r=(E_{n.f.}+E_f)/r,$$

where $E_{n.f.}$ is the ‘ non-field ’ force, and E_f is the ‘ field ’ force, given here by the negative gradient of the potential function. We may solve this equation for $E_{n.f.}$ obtaining $\mathrm{E}_{n.f.}=ir-E_f=ir+\dfrac{dV}{dx}$.

We may substitute this expression at once in equation (9) obtaining,

$$\frac{dU}{dx} = \sigma \frac{d\tau}{dx} - E_{n.f.}$$

U is known and given by equation (7). Substituting for U and solving for $E_{n.f.}$ gives, after a simple reduction,

$$E_{n.f.} = -\frac{d\tau}{dx}\int_0^\tau \frac{\sigma}{\tau}\, d\tau \quad . \quad . \quad . \quad . \, (11)$$

This 'non-field' force is located only in the body of the metal where there is a temperature gradient and does not occur at the junction between the two orientations. This is already implied in the fact that there is no electric double layer or jump in potential at the interface. The localisation of $E_{n.f.}$ is thus quite different from the localisation of the energy intakes by the current by means of which the current is maintained. For the energy intakes are, for normal substances, almost entirely situated at the interface, where the energy intake is equal to the Peltier heat. There is also an energy intake in the body of the metal equal to the Thomson heat, but this is in general smaller than the Peltier intake. These energy intakes, which as a whole maintain the current, are obviously closely related to what is called an electromotive force in simple situations, such as the battery already considered. Let us define a corresponding electromotive force in our thermocouple. The total electromotive force in the circuit is usually defined as the total energy delivered to the current by outside sources when unit quantity of electricity flows around the circuit. Here the outside source of energy is thermal and the total electromotive force is the total heat absorbed. The precise meaning of 'outside sources' in this definition requires some elucidation. The Joulean heat generated by the current and imparted to the material of the conductor might, in a literal application of the definition, be taken as an 'outside source' of energy (negative). However, this Joulean heat dissipated by the current is not regarded in the definition as part of the 'outside source' but is treated as energy received from the source and dissipated through the agency of the current. Neither is the energy received in any element of the current by the Poynting vector understood in the definition as an 'outside source,' energy on the Poynting vector being a 'field' energy, whereas the 'outside source' is a 'non-field' source.

The total electromotive force of the circuit, which may be referred to in the conventional way as total E.M.F., must be due to action in the parts of the circuit. It is one of the tasks of a complete theory of our system to find explicit expressions for this localised action. At the junctions there is an intake of heat energy, and consistently with the definition of E.M.F. for the complete circuit we localise in each junction an E.M.F. equal to the Peltier heat at the junction. Similarly in the body of the conductor where there is a temperature gradient there is a heat intake distributed along the length of the conductor, and equal per unit length of the conductor, to $\sigma \frac{dx}{d\tau}$. We recognise, therefore, in the body of the conductor, a *distributed* electromotive force, which delivers so much energy per unit length (and per unit quantity of electricity). Since this source delivers energy per unit length, its dimensions are different from the total E.M.F. We may call this a ' distributed E.M.F.,' and denote it by e.m.f., the small letters calling attention to the difference in dimensions.[1] The dimensions of E.M.F. are the dimensions of e.m.f. times length. We have explicitly,

$$\text{e.m.f.} = \sigma \frac{d\tau}{dx}. \qquad . \quad . \quad . \quad . \ (12)$$

It is to be noticed that the e.m.f. in the body of the conductor is not equal to the $E_{n.f.}$ at the same point, but in general has a different sign. There is, however, a relation between e.m.f. and $E_{n.f.}$ for the complete circuit, for it is easy to show that $\oint \text{e.m.f.}\, ds = \oint E_{n.f.} ds$. The detailed situation in a thermo-couple is thus completely different from the simple situation inside a battery, where elementary analysis shows that $E_{n.f.}$ is identically equal to the e.m.f. at all points within the battery and also at outside points. This has, I believe, been assumed to be the case in general. It certainly was the assumption made by Heaviside. It is evident that here we have something more general than is contemplated in the classical scheme of description of phenomena within massive conductors as exemplified in the field equations. What was not contemplated in the classical scheme was that the current might receive ' non-field ' energy in one locality and transport it and dump it in another. This is shown by the equation obtained from (11) and (12)

---

[1] In my book *The Thermodynamics of Electrical Phenomena in Metals*, New York, 1934, a different notation was used for these two quantities. What is here called $E_{n.f.}$ was there called $(\text{e.m.f.})_d$ (' Driving electro-motive force ') and what is here called e.m.f. was there called $(\text{e.m.f.})_w$ (' Working electro-motive force ').

$$\text{e.m.f.} - E_{n.f.} = \frac{dU}{dx} \quad . \quad . \quad . \quad . \; (13)$$

There was no place for such an energy in the classical scheme, the only (recognised) energy of the current being the field energy, the transformations of which were described by the Poynting vector. With recognition of the electron character of an electric current a place became apparent for such a non-field energy in the kinetic energy of the electrons, and a sufficiently acute realisation of the consequences of this might have led to a re-examination of the classical fundamentals.

The total energy, U, convected by the current contains, according to equation (7), a constant of the material. Subtracting off this constant, it is natural to describe the part $\tau \int_0^\tau \frac{\sigma}{\tau} d\tau$ as 'thermal energy,' since thermal energy enters the system in this amount. Denote this by U_τ, where

$$U_\tau = \int_0^\tau \frac{\sigma}{\tau} d\tau \quad . \quad . \quad . \quad . \; (14)$$

Let us examine further the consequences of 'saying' that this energy convected by the current is 'thermal' energy. Any reversible transformations of this energy within the system should be subject to the second law of thermodynamics. In the body of the metal there is a temperature gradient and a reversible Thomson heat and the second law should have something to say. Apply the second law to the volume element of Figure 4. The fraction $\triangle\tau/\tau$ of the heat convected into the region by the current at the higher temperature should be reversibly transformed within the region. Comparison shows that this is exactly the amount calculated above which is converted. The point of view is thus checked.

To give the final touch of confirmation to the consistency of this point of view, it should be possible to associate an entropy flow with the current. Consistency with the point of view of the first lecture would then demand that there be a connection between the net entropy flux out of any region and the creation of entropy by irreversible processes within the region. If U is properly described as 'thermal energy,' then we must 'say' that the current i convects with it the entropy Ui/τ or $i\int \frac{\sigma}{\tau} d\tau$. Consider now the element of volume of

Figure 4. There are two entropy fluxes, that of the electric current above, and that due to thermal conduction and equal to $\vec{q}/\tau$, where $\vec{q}$ is the thermal current, here equal to $-\kappa\frac{d\tau}{dx}$. The net entropy leaving due to the flux per unit length is (d/dx) (Flux). Inside unit length two irreversible processes occur which create entropy. Joulean heating, which create entropy at the rate i^2r/τ, and thermal conduction, which in general creates entropy at the rate $\vec{q}\,.\,\text{grad}\,\tau/\tau^2$, which here is equal to $\kappa\,(d\tau/dx)^2/\tau^2$. We then have the equation,

$$\frac{d}{dx}\left[i\int_0^\tau \frac{\sigma}{\tau}\,d\tau - \frac{\kappa}{\tau}\frac{d\tau}{dx}\right] = \frac{i^2r}{\tau} + \frac{\kappa}{\tau^2}\left(\frac{d\tau}{dx}\right)^2.$$

Reference to equations (8), (9) and (10) shows that this condition is identically satisfied.

Our verbal demands have therefore led to a completely consistent paper-and-pencil scheme. Furthermore, most of the constructions of our paper-and-pencil scheme, such as $E_{n.f.}$, e.m.f., and U_τ, make direct and unique instrumental contact. This is highly gratifying and suggests, perhaps, that we might hope to find the counterparts of these things if we could extend our theory so as to give a detailed mechanism for them. This has indeed been done by Houston,[1] who has worked out the electron theory of thermoelectricity, and found the specific electron mechanism responsible for each of these three things, $E_{n.f.}$, e.m.f., and U_τ.

Finally, it remains only to drop the restriction to single crystals which has applied to our discussions since page 150. If we carry over our scheme without alteration, using all the same letters and the same formulas, with merely the addition of a subscript to the various pertinent quantities, such as P or σ or U_τ, to denote the particular metal we are dealing with, we shall obviously still have an internally consistent scheme, which satisfies all our verbal demands and the laws of thermodynamics. In particular this will mean that in the general case of two different metals there is no potential jump at the interface corresponding to the Peltier heat, contrary to Heaviside. If the argument is re-examined it will be seen that this is basic to the whole treatment, and that without it our new constructional quantities could not have been made to fit into an edifice consistent with each other and thermodynamics. There is only one essential difference between two different metals and two orientations of the same crystal, namely that in the expression for the total energy convected by the current as given by equation (7), a constant of the material now appears which may be different for two different metals. This means that when a current flows across an interface there must be an energy influx to provide for the difference of U due to the two different metals, and this energy influx *is not thermal.* The only other possible source of energy influx is on the Poynting vector, which did not exist at the junction for two orientations of the same metal. This means that in the case of two different metals we must recognise the possibility of a double layer at the interface and a corresponding jump of potential. However, there is no thermal involvement of this double layer or its forces. We are here concerned with an aspect of the Volta effect, which we have already seen involves a possible double layer on any of three surfaces, subject to only a single condition, making it impossible to uniquely fix the potential jump at any face and in particular at the interface between the metals. This will remain as an essential indetermination until new types of experimental effects are discovered and taken into consideration. Until then, surface double layers and potential jumps do not have full instrumental status but retain some of the character of pure constructions.

---

[1] W. V. Houston, *Journal of Applied Physis*, 1941, 12, 519-529

In conclusion, in this lecture we have seen how our paper-and-pencil manipulations and verbal demands have led to constructions to which, in most cases, we have eventually been able to give complete instrumental status. I think this is characteristic of most scientific enterprise. We employ our instrumental and our paper-and-pencil and verbal operations together in a way to mutually reinforce and supplement each other. In this way new insights into the experimental domain may be indicated which may later be verified by the experiments thus suggested. In fact the two sorts of operations are so closely intertwined that it seems impossible to separate them sharply and indeed it would probably be meaningless to attempt to do so.

P. W. Bridgman

DISCUSSION

The Hypothesis of Cybernetics

In his paper on 'The Hypothesis of Cybernetics,'[1] Mr Wisdom quotes Wiener and Rosenblueth as maintaining that the 'oscillation' characteristic of purposive movements in cases of disease of the cerebellum is to be taken as the expression of 'undamped feed-back' in the nervous system, and as further suggesting that the main function of the cerebellum is 'the control of nervous feed-back mechanisms involved in purposive motor activity.' Mr Wisdom concludes that Wiener and Rosenblueth have here found important confirmation of the cybernetic hypothesis, and he adds that 'non-oscillatory motion, of course, characterises ordinary controlled action.'

I submit that there are several fallacies involved in these conclusions. The phenomena of cerebellar ataxy are extremely complex and to subsume them under the term 'oscillation' is not correct in any save the loosest descriptive sense. As analysed, this disorder of purposive movement is not an oscillation, if this term means an active to-and-fro rhythmic movement round a mean position, each component being an active one.

Even in its simplest form, as involving eye movements and *apparently* an oscillation, the deviation of the eyes to right or left is an active movement, followed by a *passive* recession to the starting point. It is not a swing round a central point, while in the case of limb movements, the disorder—according to the test employed to reveal it—varies greatly in form.

The interpretation of what is known as cerebellar ataxy (i.e. the disorder of purposive movement ensuing upon destroying lesions of the cerebellum) in terms of normal cerebellar function has long been a favourite exercise with neurologists, and the type of explanation offered by Wiener and Rosenblueth differs only in terminology from many offered before.

When the subject of this disorder reaches out with his hand to grasp an object he may overreach the mark. He is then said to have 'hypermetria,' and this has been put down to the absence of a normal 'braking' action ('Action frènatrice' of the French authors) of the cerebellum. But he may equally well underreach the mark, and he is then said to have 'hypometria,' due to the lack of the normal 'reinforcing' action of the cerebellum upon the spinal centres.

---

[1] This *Journal*, 1951, 2, 1 ff.

Of explanation of this naive order there is no end. They all fix upon the results of some purely empirical test, neglect all other observations, and build upon a fragmentary observation a general hypothesis of cerebellar function. To invoke the loss of a feed-back mechanism is merely a new *ad hoc* 'explanation' of this kind, based, in this instance, upon a false analogy between cerebellar ataxy and 'oscillation.'

Further, when we study the consequences of a lesion of the cerebellum, whether the lesion be due to disease or to some experimental operation, we may only relate the result to the operation which led to it, we may not assume that the disorder we see is some direct and clear revelation of an essential property of the cerebellum. In other words, we may not convert symptoms into normal functions by any verbal exercise, or assume that, for example, cerebellar ataxy is due to the absence of some mechanism which normally 'prevents' ataxy. The principle involved in the 'operational view'[1] applies here.

Secondly, when a true oscillation does occur in disease of the nervous system, its form is profoundly different from that of cerebellar ataxy, and the seat of the destroying lesion is within the basal ganglia of the cerebral hemispheres. The disease known as paralysis agitans (Parkinson's Disease) is the familiar example of this.

Finally, all purposive movements in normal persons *if sufficiently slow* show a fine oscillation of a rate varying, from individual to individual, between 8 and 12 per second. Schafer's *Textbook of Physiology* (1900) gives several myograms showing this quality of slow movement. Later myographic studies of paralysis agitans[2] show that as this malady develops and the muscles become progressively more rigid, the oscillations slow down in rate and increase in amplitude, and persist until purposive movements are carried out at speed, when they may often disappear and smooth movement develop. There is a gradual transition between the fine oscillation of slow movements in the normal persons and the gross tremor of paralysis agitans. Therefore, the statement that normal movement is non-oscillatory requires an important qualification. Unqualified, it is not accurate.

---

[1] Cf. Dingle, this *Journal*, 1950, 1, 1 ff.

[2] Walshe, *Lancet*, 1929, 1, 693

If cybernetics is to aid us in the physiological interpretation of disorders of movement, and thus in the determination of normal neural processes, it must base its argument upon correct assessment of the particular disorder of movement under consideration, it must observe the principle implicit in the ' operational view', and it must not perpetuate the ancient error of confusing symptoms with functions.

F. M. R. WALSHE

REVIEWS

Science : its Method and Philosophy, G. Burniston Brown, George Allen and Unwin, Ltd., London, 1950. Pp. 189. 15s.

Dr Burniston Brown writes as a physicist explaining his problems to the general reader, but he has tried to do more than this, and ' in a way which, I think, most scientists would accept if they thought about these matters.' That they think about them less than they might is doubtless due to the prestige of a vague something called ' philosophy,' or to a still vaguer suspicion that ' symbol-situations ' (including semantics or the like) may trip them up. Dr Brown is fortunately afraid of neither, and he takes the offensive with engaging modesty. The result is a valuable contribution to our understanding of the purpose and procedure of science—clear, entertaining, and original.

It is a welcome characteristic of the treatment adopted that although mathematical and statistical methods are not neglected, the book starts with the nature of learning rather than with measurement. The behaviour of animals prepares us to understand both the requirements of man as a communicating animal, and the peculiarities of words and symbols as the tools which have made possible the communication and advance of knowledge.

This laudable concern with language and communication leads to a declaratory definition of a ' fact ' as a proposition (a verbal assertion) which can be verified. That leaves us in some doubt on the referential side—concerning the status of what we observe or find (pp. 40 and 178) in relation to what we are certain of, i.e. to events of consciousness (pp. 37 and 172), or the private knowledge of which public knowledge is said (p. 43) to be part. On the definitions proposed, such a slip as ' statements of fact ' (p. 180) requires a verbal correction ; otherwise, consistency can doubtless be attained at the expense of current usage.

Though this section of eleven pages occupies less than 6 per cent of the book, it deserves more attention than academic critics are likely to give it. The author's objection to current usage, which treats ' fact ' as an abbreviation ' for what makes an assertion true,' or the verified referent of a symbol, is due in part to his rejection (p. 152) of the metalanguages which are supposed by logicians to infest the background of such a formulation. Moreover, in the current usage of scientists, facts are the certainties (what is the case) which hypotheses and theories are invented to explain ; and when these explanations (which are uncertain) are themselves regarded as ' what is the case,' confusion is likely to arise.

It is just thirty years since the late Professor G. H. Hardy projected 'Mathematics for Philosophers'—a propaedeutic which never saw the light. Today such a theme would have to range far beyond logistics and its borderlands, to cover Milne, Eddington, and a dozen living influences on the field once monopolised by a Kant or a Bradley. Meanwhile, the special sciences have been resolutely expanding their frontiers with more than global implications ; and the public imagination has recently been directed outwards, to the self-creative concept of hydrogen in a whirling pluriverse (Hoyle) ; and inwards, to the cosmic pervasiveness of encephalic patterns (Young). So the attempt to provide an historical perspective is timely.

The lives and achievements of Aristotle, Bacon, and Whewell, the three outstanding methodologists who have produced an *Organon*, are the subject of first-hand studies ; Newton is allotted twenty-five pages as 'the greatest scientific genius that has appeared up to the present time' ; and a critique of Mill prepares the way for a detailed study of the model researches of W. C. Wells (1757-1812) on dew. After a final excursus on 'Ultra-Modern Science,' introducing the reader to the unanswered questions raised by Eddington and Milne, the argument is epitomised and further elucidated in a discursive dialogue, continuing *mutatis mutandis* that of Galileo (1632).

The conclusion that 'science is good for the health and happiness of mankind,' and that, if all men hated error as strongly as the scientists, they might be fit to control the power put into their hands, makes our present predicament the more regrettable. For if scientists inadvertently explode the Universe before they have had time to explain it, we shall never know what is the case.

C. K. Ogden

Can We Agree ? Chauncey D. Leake and Patrick Romanell, University of Texas Press, Austin, 1950. Pp. xiii + 110.

The sub-title of this book, 'A Scientist and a Philosopher Argue about Ethics,' accurately describes some 67 of its 110 pages. The remainder consists of a 'Retrospective Introduction' by Dr Leake, which should be read last as well as first, an essay by Dr Romanell on 'A Naturalistic Logic with Metaphysics,' and one by Dr Leake (privately printed in 1940) on 'Science implies Freedom,' together with an index which is almost a concordance. There is a Foreword by Albert P. Brogan.

The argument itself consists of four essays (enlivened by some pertinent quotations) entitled 'Ethicogenesis,' Dr Leake's address as retiring Vice-President, Section L-History and Philosophy of Science, A.A.A.S. (1944) ; 'A Philosopher's Reply to a Scientist's Ethic,' by Dr Romanell (1945) ;

a rejoinder by Dr Leake, ' A Scientific versus a Metaphysical Approach to Ethics ' (1946) ; and a surrejoinder by Dr Romanell, ' A Naturalistic versus a Positivistic Approach to Ethics ' (1948).

The authors rightly admit that their argument is neither comprehensive nor conclusive, and that it was not intended to be so. They hope that it may be provocative of further discussion between scientists and philosophers, the explicit inference being that a ' prime requisite ' for human survival is that ' agreement must quickly be reached between our intellectual leaders on the ethical implications of our scientifically verifiable knowledge of ourselves and our environment ' ; which must then be translated into language suitable for a public educational drive (Dr Leake's comment, not the reviewer's). As Dr Romanell pointed out, there is disagreement between himself and Dr Leake, not only on (1) the nature of scientific method, (2) the nature of ethics, and (3) the nature of metaphysics, but also, more radically, on their ultimate vision of ' the good life.' The discussion in this book certainly helps to clarify definitions. It is always as well to make sure that we mean the same things by the same words, which, in the case of the two authors, was not so at first. They do not seem, in the course of four years, to have got very much forrader. In view of the fact that those four years have seen the dropping of two atom bombs and the steady deterioration of world relationships, one is tempted to feel that the human race has not much chance of survival if the prime requisite is agreement on an intellectual level. Fortunately the common man often has more common-sense than either the scientist or the philosopher, and the instinct for survival may guide him to agree with his adversary quickly, while his intellectual ' betters ' are still busy with their dictionaries.

This is not to say that the book is not interesting ; it is. There are scores of statements made that the reviewer would like to argue with the authors ; but they also score some very good points against each other ; so much so, that one is tempted to echo the remark of the Mayor to the candidates in a recent Parliamentary Election : ' May you both win, gentlemen ! ' And they do make it clear that it is helpful for scientists and philosophers to get together, if only to understand their own limitations. Perhaps if, instead of waiting for full agreement, they could at least agree on a policy of public education based on one of the articles of faith recommended by Dr Leake : ' that truth cannot be established by compulsion, nor error overcome by force,' they might be more socially effective than appears likely at present.

Kathleen Lonsdale

Aquinas and Kant, Gavin Ardley, Longmans Green & Co., London, 1950. Pp. x + 256. 18s.

THE author of this book is greatly perturbed about the ultimate basis of our knowledge of the universe, and the conflicting character of modern thought in philosophy and physics. And well he may be. The rise of Neo-Thomism in one form or another is a feature of our generation. No less marked, however, is the advance of theoretical physics associated with the names of Poincaré, Eddington, and one or two others of comparable calibre. Again, as Mr Ardley remarks, St Thomas Aquinas and Kant seem strange bedfellows indeed, as Aristotle and the Fathers were aforetime. Observing that the latter pair were eventually 'reconciled,' he believes that a corresponding state of bliss for the former couple is only a matter of time. Kant's idea of a physicist was that of an extremely active person, by no means content to receive laws from nature, but perpetually engaged in the task of formulating laws of his own which he 'fastened' upon nature, and to which she was obliged to conform. All that is said about the Procrustean bed and the chopper is most apt, and indeed on this view, deserved. Nevertheless, according to Mr Ardley, it is a grave error to imagine that this coercive technique is intrinsically necessary ; it is merely a device to secure power for mankind.

Over against this stands metaphysics in serene detachment, ready as always to admit the practical advantages of 'saving appearances,' whether in classical physics or in modern metrical technology, but claiming the absolute title to the possession of philosophical truth. Seldom has the precept 'between us and you there is a great gulf fixed . . .' been restated in starker form. Why, therefore, it is asked, are we in fact confronted with physics heaping triumph upon triumph in almost every department of twentieth-century life ? Mr Ardley replies in effect that had a divergent system of 'categorisation' been set up, things might have worked out differently. This *riposte* is very disappointing, being nothing short of wholly irrelevant, since what we want to know is why physics, as commonly understood, should be any good at all.

No reasonable person has anything but reverence for the *philosophia perennis*, yet this book cannot be said to have helped to bring the natural sciences of to-day within its broad and generous frontiers. Unfortunately, too, Mr Ardley's style lacks attractiveness ; it is rather that of a school-teacher admonishing an unwilling class, and underlining for them, as he goes along, what they are meant to learn by heart.

IAN RAWLINS

Modern Science and its Philosophy, Philipp Frank, Harvard University Press, Cambridge (Mass.), 1949 (London: Geoffrey Cumberlege, Oxford University Press). Pp. 324. 30s.

This book consists of fifteen essays which have appeared in various journals during a period extending from 1907 to 1947, although only two bear a date before 1930. Eight of these have already been published in book form in *Between Physics and Philosophy*, published in 1941. To these is added, by way of introduction, an interesting account of the history of the Vienna Circle. Two essays are devoted to an exposition of the views of Ernst Mach, ten to the various so-called philosophical interpretations of physical theory, including an interesting article on logical empiricism and the philosophy of the Soviet Union, and a good account of the philosophical discussions occasioned by the Copernican revolution. Finally, the book concludes with two articles on present defects in the education of physicists and the difficulties of overcoming them, and on the place of logic and metaphysics in the advancement of modern science.

The philosophical problems discussed in these pages are the following: (i) What exactly do physicists observe? (ii) How is what they observe related to the statements which they write down in their laboratory notebooks? (iii) How are the latter statements related to the theoretical statements of physics? and (iv) What part do logic and mathematics play in the construction and testing of physical theories?

The answers which Dr Frank has given to these questions in his 'Foundations of Physics' in the *Encyclopedia of Unified Science* are much clearer than those in the present volume. The latter appears to be intended for a wider circle of readers, and it is extremely difficult to be both popular and precise. Moreover, owing to the widely different times at which the articles have been written, there is a considerable diversity in terminology which will be rather bewildering to the reader who is not familiar with the changes that have taken place in the views of logical empiricists during the period covered by these essays. Some obscurities would have been avoided if the distinction between object language and metalanguage had been explained and used. Of the four problems mentioned above, (iii) and (iv) seem to have been more successfully dealt with than (i) and (ii). In particular Dr Frank's explanations of Mach's views do not meet all the difficulties which have been felt regarding them.

These problems will never be properly cleared up until someone carries out the laborious task of making a complete formalisation and axiomatisation of a small part of physical theory, showing, in minute and convincing detail, every step of the procedure from the laboratory bench to the fully elaborated theory. The Polish methodologists have shown us how this is to be done. But one curious feature of this book is the complete absence of any mention of the great Polish school of methodology which flourished between the two world wars and contributed so much to the clarification of the problems discussed in these essays and greatly influenced some members of the Vienna Circle. The names of Kotarbiński, Leśniewski, Lukasiewicz and Tarski do not appear in its pages.[1]

Nevertheless this book contains much interesting reading matter and may be warmly recommended to students of the history of science.

J. H. WOODGER

The Revolt Against Reason, A. Lunn, Eyre & Spottiswoode (Publishers), Ltd., London, 1950. Pp. 252.

THE taste for controversial literature, widespread in the seventeenth and eighteenth centuries, has so far diminished now that a would-be controversialist, desiring to woo the attention of the reading public, has usually to disguise his work as a novel or a scientific or philosophic treatise. Mr Lunn, however, disdains any such subterfuge ; he writes as a controversialist with a point to make, namely that materialism, whether vulgar or dialectical, behaviourism and logical positivism are destructive of their own logical coherence, and entail a simultaneous breakdown in moral values.

Though his theme is single, Mr Lunn treats its subordinate aspects rather unevenly. The earlier chapters of his book outline the birth of scientific inquiry and the development of the scholastic method that formed the matrix of scientific ratiocination. The attack by Luther on reason is then described together with the concurrent moral disorders of the German Reformation period. Several chapters are devoted to the rise within science of an anti-religious bias, which gained so greatly from post-Darwinian evolutionary thought, and in this connection Mr Lunn includes an excursus on the evidences for biological evolution by which he is not greatly impressed. As a counter-blast to scientific materialism, a chapter is introduced on the evidences provided by psychical research, the author remarking, correctly enough, that these are more likely to interest inquirers today than philosophic arguments against materialism, however closely reasoned. The final chapters deal rather cursorily with materialist psychology, the treatment of science in the Soviet Union, the less pleasant traits of surrealism and logical positivism.

It is, of course, impossible in one book to deal exhaustively with all these topics. Mr Lunn, however, does succeed in his main objective of exposing some of the logical inconsistencies in the views he combats, and of pointing out how they subvert morality. Another rather unpopular point which the author makes, is the extent to which the philosophical, religious or anti-religious views of scientists colour their own scientific writings. Only those with no scientific contacts can minimise the force of this observation. Mr Lunn is certainly correct, too, in claiming that many of the views he combats have gained popularity through their approbation of departures from traditional morality.

---

[1] A good account of their work is given in *Polish Science and Learning*, No. 6, Oxford University Press, 1945, price 2s. 6d.

Mr Lunn is at his best when dealing with general principles. His treatment of more concrete issues tends to be prejudiced by insufficient cover of the relevant data. Modern evolutionary theory, for instance, is largely based on genetic grounds. Mr Lunn does not consider these, though he refers repeatedly to the dicta of various scientists, including Darwin himself. Evolution, however, is an induction from evidence and it is quite irrelevant to consider personal views, many of which, including Darwin's, are quite obviously false and of no more than historic interest. As regards the objections that Mr Lunn puts forward against evolution, it is relevant to observe (1) that the choice between asexual and sexual reproduction may be determined by a simple gene difference in many higher plants, (2) that complex organs are usually regarded as having evolved from more rudimentary organs, not from undifferentiated tissues, (3) that single mutants have been observed so fundamental that were their origin unknown they would have been classified in a separate family from their parents ; (4) that while flight may be a ' hit or miss ' phenomenon, the structural modifications that led up to it may have been relatively gradual ; (5) that nascent bony structures are known from the fossil record ; and (6) that the affinities of *Archaeopteryx* with both reptiles and birds cannot be dismissed by a quotation from Berg.

Mr Lunn's book would also benefit from a more thorough proof reading, and in this connection surely ' how ' and ' why ' have been mixed up in the fourth paragraph on p. 68.

R. H. Richens

The Philosophy of Mathematics, Edward A. Maziarz, Philosophical Library, New York, 1950. Pp. viii + 286. $4.00.

The author of this book means by the philosophy of mathematics the ' field of philosophical enquiry concerned with the nature of mathematical abstraction.' In an introductory chapter he seeks to place the subject ' in a wider speculative setting.' The rest of the book is then in two parts.

Part I is on ' The history of the philosophy of mathematics.' This again starts with an introductory survey on the historical relation between mathematics and philosophy. The following chapters deal in turn with ancient mathematics and philosophy, the ' Cartesian era,' the British empiricists, the idealism of Kant and the positivism of Comte, and finally, in more general terms, with modern trends.

Part II is called ' The philosophy of mathematics.' In its first chapter the author presents a classification of knowledge in general and then proposes to classify the ' speculative sciences ' into *physical science* characterised by ' total abstraction,' *mathematical science* characterised by ' formal abstraction,' and *metaphysics* as ' the most perfect manner of knowing of which the human mind is capable.' The next two chapters deal more fully with mathematical abstraction. Here and elsewhere in the book there are brief commentaries upon the three contemporary views of the nature of mathematics—logicist, formalist, and intuitionist. But the author is mainly concerned with what he means by mathematics as an abstractive science and the consequent status of what he calls the ' mathematical universe.' So far as I understand him, his attitude is simply that mathematics has a foundation in man's common experience of an external world, that it is reached by one particular kind of activity of the mind in response to this experience, that this kind of mental activity is distinguishable from, but related to, other discernible kinds, and that there is a unifying activity which is metaphysics. Together with reiterated criticisms of tendencies to regard mathematics either as being without foundation in extra-mental experience or as being more comprehensive than he allows, this I think is the position summarised in the author's concluding chapter. He expresses it, however, in very different language.

At the outset one becomes sympathetically disposed towards the author because of the evident sincerity of his conviction that he has something worth saying, and because he expresses himself with such spontaneous fluency. Also, what appears to be his main thesis, that an adequate philosophy of mathematics is to be attained only by due regard to the place of mathematics in thought in general, and his implied view of the significance of the historical background are unexceptionable.

Unfortunately, the sympathetic reader is likely to be disappointed later on. For the book turns out to be rather one *about* the philosophy of mathematics than an exposition *of* the subject. The author does not get down to any systematic analytical study. Indeed, so far as I can penetrate what is to a mathematician a haze of metaphysical terms, the author's views upon the nature of physical science and of mathematics appear to verge upon the naive. Also he seems to employ only preconceived and undefined meanings for many of his general notions such as ' knowledge,' ' reality ' and ' existence.'

Nevertheless, no review of the book should fail to praise the care with which it has been documented. With its copious references and bibliography it can form a valuable guide to the literature of the vast field with which it deals.

W. H. McCrea

A Critique of Logical Positivism, C. E. M. Joad, Victor Gollancz, Ltd., London, 1950. Pp. 154. 10s. 6d.

During the last twenty years the so-called logical positivist has risen in the philosophical world from *enfant terrible* to whipping boy. No one quite knows what is meant by ' Logical Positivism '; but everybody is against it. This makes the philosopher's game more amusing since he can so set up a scarecrow as adversary and knock it down again to display his prowess. Dr Joad plays this game with gusto, taking the first edition of Ayer's *Language, Truth and Logic* as a specimen of Logical Positivism.

There is no doubt that the ideas contained in that book require criticism, some of which Professor Ayer has indicated in the preface to the second edition. Dr Joad's critique can thus be only a post mortem—an historical task ; and to assess his critical efforts would need detailed confrontation with Ayer's book. But what makes Joad's essay of general interest is that it transcends the original objective.

Historically, Logical Positivism was the attempt to apply modern logic and scientific method to the problems of philosophy, a successor to the older kind of empiricism as Joad points out. This is, of course, why Logical Positivism is unsatisfactory today ; it remained within the confines of traditional epistemology. When it was seen that scientific method tolerates no -isms, the early formulations of the Vienna circle (usually spoken of as Logical Positivism) were changed. This illustrates the scientific attitude of always revising and, if possible, of improving our theories. Curiously enough it appears to irritate Dr Joad. While imputing dogmatism to the logical positivists, he complains that they have now re-formulated their main statements so as to make them nearly acceptable. It is true that the original pronouncements of logical positivists were needlessly provocative ; but this is not the reason why the mere mention of ' Logical Positivism ' makes people see red.

It is that ' under its influence young men and women confidently affirm that there are no absolutes, that metaphysics is nonsense, that the scientific is the only method which reaches valid results and that the order of reality which science studies is the only order that there is ' (p. 10).

And here is the main reason for Joad's, and everyone else's, attack (and the only reason for taking the trouble of reviewing this book) : the dislike of logic and of science. It is shown also by the fact that the three main ideas under attack are exactly those which, though certainly in need of a more precise formulation, represent genuine achievements closely related to modern science. To put it briefly : (1) The verifiability criterion of meaning is surely acceptable though its epistemological form must be abandoned. It is neither a thesis, nor a principle, nor a law, nor a prohibition, not even simply a definition ; but an attempt of reconstructing in logical terms the use of factual statements, both within the context of science and of everyday life. There is a problem of adequacy, but not of truth or of metaphysical principle ; the discussion in Joad's book does not make this clear. (2) The rejection of metaphysics, I agree, is sometimes made in too sweeping a manner—for the reason that metaphysical sentences are so obscure and metaphysicians differ so much with one another. Yet, as Dr Joad correctly says, certain metaphysical sentences are claimed as factual but cannot be verified by experience. That is to say, we are expected to accept sentences about matters of fact that are, in principle, not verifiable by any fact. We are asked to use sentences for which no (semantic) rules of usage can be given. It is no wonder that logical or factual meaning cannot be ascribed to such sentences, when taken literally. It does not follow, however, that sentences of this sort are unimportant ; they have often an enormous emotional import (e.g. in theology), and this has never been denied. (3) Finally, the emotive theory of ethics is claimed to undermine morality and lead to Fascism. ' Logical positivist conclusions really do eviscerate the universe ' (p. 19). This furnishes the recurrent theme of Joad's book. It is a touchingly old-fashioned argument for which, however, I am grateful to Dr Joad that, today, he still feels concern about Fascism. But since 399 B.C. this kind of reasoning has often been used against philosophic radicals and, as one must admit, not always with success. For historical accuracy it must be said that the Continental adherents of what is loosely called Logical Positivism have been, as far as I know without exception, political refugees from Fascism ; and today their writings are banned in Soviet Russia. One could not wish for a better recommendation.

But to come to a more serious argument : Dr Joad says that to accept the emotive theory ' is to deprive both ethics and religion of emotive significance ' (p. 145). In other words, to say that ethical sentences are primarily used to express emotion is to say that they are emotionally not significant ! The emotive theory is anyway not quite so simple since ethical sentences are used within a context and so are related to factual sentences. The theory emphasises the different usage to which we put ethical and factual sentences and attempts to analyse the meaning of the terms involved. To investigate what we mean by ' right ' and ' wrong,' or by ' value,' does not imply that we must not use these terms nor that sentences containing them have no import. To say ' Do not kill ' is not to describe anything but to exhort us to act in a certain manner, and so the problem of truth does not arise ; not even in the sense of the Correspondence theory of truth which Joad holds. For who would affirm that we accept this command because it corresponds to a fact ?

No, this won't do. Dr Joad does not analyse Professor Ayer's views, nor lay the ghost of Logical Positivism. Meaning analysis with the help of logic and of scientific method is condemned, on emotional grounds. What a pity that Dr Joad did not heed the advice of the motto which he has made his trade-mark 'It all depends what you mean.'

E. H. Hutten

BOOKS RECEIVED FOR REVIEW

Inclusion of books in this list does not preclude their being reviewed in later issues

Actas del Primer Congreso Nacional de Filosofía—Mendoza Argentina—Marzo 30-Abril 9, 1949. Universidad Nacional de Cuyo, Vol. I, pp. 1-632, Vol. II, pp. 633-1412, Vol. III, pp. 1413-2200.

Oskar Becker, *Einführung in die Logistik*, Westkulturverlag Anton Hain, Meisenheim/Glan, 1951, pp. 92, DM 15.

J. Bronowski, *The Commonsense of Science*, William Heinemann Ltd., London, 1951, pp. 154, 8s. 6d.

Carola Baumgardt, *Johannes Kepler: Life and Letters*, Philosophical Library Inc., New York, 1941, pp. 209, $3.75.

Paolo Filiasi Carcano, *La Filosofia d'Oggi al Congresso di Amsterdam* (11-18 *agosto* 1948), Soc. ed. del 'Foro Italiano,' Roma, 1950, pp. viii + 144, Lire 500.

Philippe Devaux, *De Thalès à Bergson. Introduction historique à la Philosophie, Science et Lettres*, Liège, pp. 439.

Benjamin Farrington, *Francis Bacon: Philosopher of Industrial Science*, Lawrence and Wishart Ltd., London, 1951, pp. viii + 198, 12s. 6d.

Nelson Goodman, *The Structure of Appearance*, Harvard University Press (London: Geoffrey Cumberlege), 1951, pp. xv + 315, 25s.

D. Hilbert and W. Ackermann, *Principles of Mathematical Logic*, Chelsea Publishing Co., New York, 1950 (1st German ed., Berlin 1928), pp. xii + 172, $3.50.

The History of Science—Origins and Results of the Scientific Revolution: A Symposium (Broadcast Talks), Cohen & West Ltd., London, 1951, pp. 184, 8s. 6d.

Lancelot Hogben, *Science for the Citizen*, George Allen & Unwin, Ltd., London, 1951 (3rd revised ed., 8th impression), pp. 1146, 20s.

Joergen Joergensen, *The Development of Logical Empiricism*, International Encyclopaedia of Unified Science, Vol. II, No. 9, University of Chicago Press, 1951, pp. iii + 100, $1.25.

R. O. Kapp, *Mind, Life and Body*, Constable & Co. Ltd., London, 1951, pp. xvii + 196, 12s. 6d.

Morris T. Keeton, *The Philosophy of Edmund Montgomery*, Southern Methodist University Press, Dallas, Texas, 1950, pp. xi + 386, $5.00.

Edmund Landau, *Foundations of Analysis*, Chelsea Publishing Co., New Yor 1951 (1st German ed., Leipzig, 1930), pp. xiv + 134, $3.25.

Ernst Lehrs, *Man or Matter*, Faber & Faber Ltd., London, 1951, pp. 378, 30s.

Edited by A. A. Luce, *The Works of George Berkeley, Bishop of Cloyne*, Vol. IV, Thomas Nelson & Sons Ltd., Edinburgh, 1951, pp. viii + 264, 30s.

Heinrich Lützeler, *Jahrbuch für Aesthetik und Allgemeine Kunstwissenschaft*, Ferdinand Enke Verlag, Stuttgart, 1951, pp. 416, DM 35.

Andries H. D. MacLeod, *Over de verhouding van de speciale relativiteits theorie tot de Newtoniaansche voorstellingen van ruimte en tijd*, Drukkerij 'Humanitas' The Hague, 1950, pp. 37.

Jakob Mandelker, *A New Theory of Gravitation*, Philosophical Library, Inc., New York, 1951, pp. vi + 25, $2.75.

Arthur Pap, *Elements of Analytic Philosophy*, The Macmillan Company, New York, 1949, pp. xv + 526, 35s.

Angus Sinclair, *The Conditions of Knowing: An Essay towards a Theory of Knowledge*, Routledge & Kegan Paul Ltd., London, 1951, pp. 260, 21s.

I. K. Stephens, *The Hermit Philosopher of Liendo*, Southern Methodist University Press, Dallas, Texas, 1951, pp. x + 402, $5.00.

Edited by Frederick Watkins, *Hume: Theory of Politics*, Thomas Nelson & Sons Ltd., Edinburgh, 1951, pp. xxx + 246, 7s. 6d.

Hermann Weyl (Translator: Henry L. Brose), *Space—Time—Matter*, Dover Publications Inc., New York, 1951, pp. xvi + 330, $3.95.

Sir Edmund Whittaker, *A History of the Theories of Aether and Electricity*, Thomas Nelson & Sons Ltd., Edinburgh, 1951, pp. xiv + 434, 32s. 6d.

Edited by Lancelot Law Whyte, *Aspects of Form*, Percy Lund Humphries & Co. Ltd., London, 1951, pp. ix + 249, 21s.

Edited by D. C. Yalden-Thomson, *Hume: Theory of Knowledge*, Thomas Nelson and Sons Ltd., Edinburgh, 1951, pp. xxvii + 265, 7s. 6d.